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# THE PSYCHOLOGICAL REVIEW.

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## PENDULUM CHRONOSCOPES AND ACCESSORIES FOR PSYCHOLOGICAL EXPERIMENTATION.

BY JOHN A. BERGSTRÖM,

*Professor of Education, Stanford University, Cal.*

In connection with his work as director of the psychological laboratory of Indiana University, which position was held till August 1, 1908, the writer had occasion to attempt to develop chronometric apparatus both for research and for the student practice course. Partial notices of these attempts have occasionally appeared, but no complete account such as is here given. In the PSYCHOLOGICAL REVIEW, Vol. 7, No. 5, 1900, the writer described 'A Type of Pendulum Chronoscope and Attention Apparatus,' the construction of which was suggested particularly by accounts of the Fitz pendulum chronoscope. The chief difference between this and the model constructed by the writer lay in the mode of carrying and clamping the index. In this first model, the axis of the pendulum was so cut that its knife edges were exactly in line and in the middle of the axis so that a further extension of the axis, which carried the index, rotated as if the axis were in circular bearings when the pendulum moved. The index was attached to a soft iron disc, which could rotate on the extension of the axis and was pushed by a spring against a friction disc attached to the end of this extension. A magnet fixed to the bracket, on which the axis of the pendulum rotated, could be made to attract the soft iron disc carrying the index, and fix it in position. When the disc of the index rested against the friction disc of the axis, it was held in place sufficiently firmly to follow the movements of the pendulum accurately. On the attraction of the index disc from the fric-

tion disc to the magnet, the friction keeping the index moving with the pendulum was greatly reduced and the index disc was held firmly in position against the magnet, with its point against the scale. The pendulum could swing on fairly freely and was caught at the end of the return swing by teeth on the escapement armature. The interval measured was the interval between the release of the pendulum by the escapement and the arresting of the index. The first chronoscope had mercury cup arrangements for intervals and a movable bob for securing different rates. In the later models these have been replaced by devices both more nearly accurate and more convenient. In the first model the release of the pendulum was not noiseless and was therefore defective. However, from the point of view of accuracy, aside from the work of the two devices just mentioned, the work of the first pendulum chronoscope seems to have been as good as that of later models.

To remedy the defects of the first model and to produce a more finished piece of apparatus, a second model was designed. This is the most complete form yet constructed, as the third is only a simplification of the second. Of this second model no detailed description has been published till this present article. It was constructed from the writer's designs by Stoelting and Co. of Chicago and formed a part of this firm's exhibit at the St. Louis Fair in 1904. A cut of model No. 2 appeared in the Indiana University book, published for the same fair.

In model No. 2, the release and catch of the pendulum is of the same sort as in model No. 1 except that it has been made silent by preventing the armature from touching the magnet of the escapement. As in the first model, the release and catch has several notches so that the pendulum may be caught on the return swing by some one of them, according to its retardation. In some, perhaps all other, pendulum chronoscopes, the holding and release of the pendulum for a record is secured by having a magnet hold the pendulum directly at the beginning of its swing, the pendulum beginning its swing when the current is broken. That this last form of release is also good seems evident, and which of the two types of release will be adopted in the construction of pendulum chronoscopes cannot be pre-

dicted with certainty. Both are silent, which is essential, if the experimenter would avoid unnecessary trouble and inaccuracy. However, in case the pendulum is held directly by a magnet some variation may be expected from differences in its retardation, due to differences in the duration of the current and differences in the weight of the pendulum for fast and slow rates, as well as differences in annealing. These effects are largely avoided in the escapement used. The pendulum is released by the making of the circuit of the escapement magnet which may therefore be supposed to be in the same condition for every record, and a current is used which is twice as great as is necessary to release the pendulum for the fastest rate, when it bears most heavily on the tooth of the escapement. Only the form of escapement peculiar to this instrument has been used with these chronoscopes so the writer has made no experimental comparison of the two types. However, in testing chronoscopes 1, 2 and 3 with the simple falling ball apparatus, in which the falling of the ball is comparable with the falling of the pendulum with the simple magnet escape, the chronoscopes seemed to be more accurate than the testing device, because of variations in the hysteresis of the ball and the magnet holding it. Systematic rotation of the ball, which was probably not so well annealed as it should have been, and the use of a current barely sufficient to hold the ball gave the best results.

For rapid action of the magnets, the cores must be comparatively large with few windings of wire. A convenient size of the magnets both of the escape and the index is one that will require the use of the same or of equal currents to make the chronoscope give the correct time. In the first models of chronoscope No. 2 an error was made with regard to this matter so that a stronger current was required for the escapement than for the index.

The different rates of swing of the pendulum are secured by screwing on the necessary counterpoises. In model No. 1 these rates were secured by raising and lowering an eccentric bob, which also permitted the experimenter to adjust the pendulum so that it would hang vertically. The plan of detachable counterpoises, which has usually been employed in pendulum chronoscopes, seems to be the more convenient.

Instead of the semicircular mercury cups for securing intervals, as in No. 1, a system of one fixed and two movable keys on a curved slide is employed. These are both more accurate and more convenient than the mercury cups. They can be used for definite intervals for any purpose. In the laboratory of Indiana University they have been used especially for giving the time of



FIG. 1. Model No. 2.

exposure in tachistoscopic experiments, and in standardizing the smaller pendulum chronoscopes of model 3. A laboratory equipment consisting of one chronoscope of the type of model 2 and several of the type of model 3, especially for student use, seems to meet most actual needs.

The contacts in the small movable keys are kept together by having a pointed projection of the lever making the contact just slip over a triangular tooth on a short spring. When the lever



is struck by the pendulum and is moved a short distance, perhaps less than half a millimeter, the projection slips over the tooth,

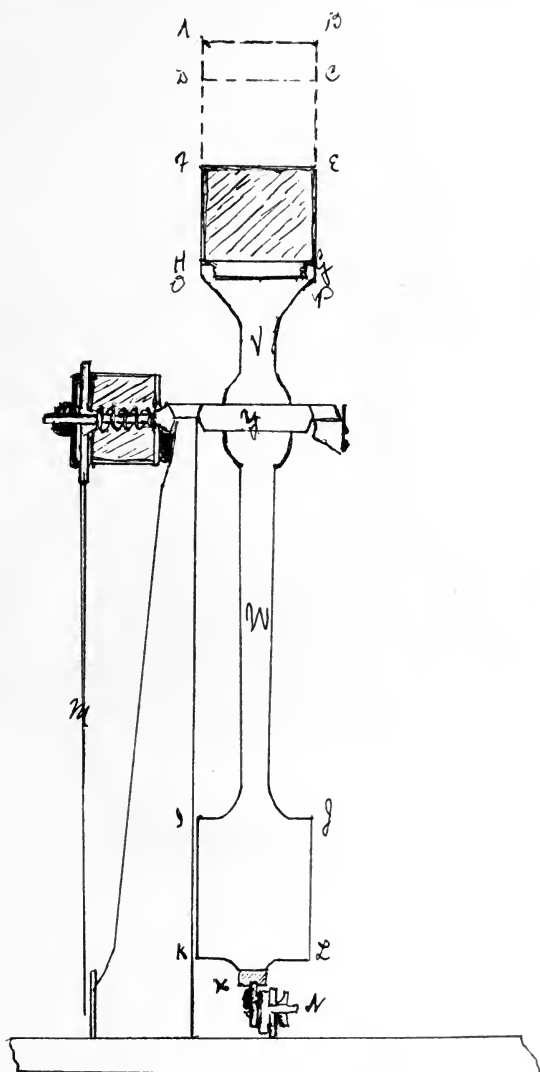


FIG. 2. Pendulum of Model No. 2.

which then throws it out of the way. By this means sufficient pressure at the contacts is secured and the resistance to the pendulum is made small.

The pendulum is so constructed that with its three counterpoises it will swing at three different rates, namely, at one half, one and two seconds for the half swing. Fig. 2 gives the proportions of the different parts of the pendulum graphically. The distance  $X-Y$  is 20 cm.;  $Y-OP$  is 5 cm.;  $OH$  is 6 mm.; the thickness of  $V$  is 1 cm.; of  $W$  1 cm. at the top and 0.8 cm. near  $I, J$ ;  $IJ$  is 4 cm.,  $JL$  5 cm.; and  $HG$  is 4 cm.;  $HF$  is 3.4 cm.;  $HD$  is 6.5 cm.; and  $HA$  is 8 cm.;  $HFEG$  is hollow with a .75 mm. wall, except at  $HG$  where the thickness on each side of the screw is 1 mm., though when it is fully screwed on,  $HGPO$  is to be practically solid. Parts  $HFEG$ ,  $HDCG$  and  $HABG$  may be changed to give respectively half swings of .5, 1 and 2 seconds.  $M$  represents the index and  $N$  a cross-section of one of the movable keys.

The length of the scale arc is about 29 cm.

To secure correct proportions in the construction of this pendulum requires considerable time and skill; and in the simplified form, No. 3, only an approximation is attempted. The calibration of the scale is done in essentially the same way in all cases, except that the procedure was somewhat simplified in connection with the construction of model 3. The calibration is both easy and accurate and consists essentially of letting the sparks from an induction coil actuated by an electric tuning fork of 100 vibrations a second run over the piece from which the scale is to be made, after it has been smoked. Not all tuning forks even by the best makers give the correct number of vibrations; different conditions must also be taken into account. Some correction of the tuning fork may therefore be necessary. The method of securing the spark records for the scale has been the same throughout, though a couple of minor improvements have been introduced. A very fine wire is run down the index and the end turned toward the scale. The upper end has a hook at the level of the axis and rests in a mercury cup. The formation of the spark circuit can easily be imagined. To make the pendulum start at a certain point between the sparks, a rather complicated system of wiring and balancing of currents was at first arranged, so that when the escape was gently shaken by the primary giving rise to a spark another current

would be switched in to pull the escape down. This plan gives good results and the spark can be run over the same record several times with coincidence. The pendulum will by this plan also start at corresponding points for different rates. However, the method is complicated and a very simple device has been introduced which serves fully as well.

With both methods the escapement is so placed as barely to hold the pendulum. With the new plan, a good-sized permanent magnet is placed over the back projection of the escapement lever so that this will fly up to the magnet when the escapement is given a very slight shake. This shake is given to it by the interrupted current of the primary and usually in the same way on the closing of the primary circuit, at least so that several coincident spark records may be obtained. The second change in the making of the spark records was that of omitting smoked paper and smoking directly the piece intended for the scale, which seems to give better results.

Two points should be noted in levelling the chronoscope. Each instrument has a universal level attached. By the aid of this it can be approximately levelled, but this procedure should be supplemented by seeing that the index runs the full length of the scale. With the combination of these two ways errors from levelling should be very small.

Nearly all the different time measurements fall conveniently within the limits of the forward swing one scale or the other. Occasionally a record is longer, in which case the time is ascertained by noting the point at which the index stops on the return or subsequent swings.

The three scales are not equally valuable, the half second and two second scale being more serviceable. In model No. 3 only these two are used and the one second scale could be omitted in model No. 2, but it seems sufficiently useful to be retained in the complete instrument.

Fig. 3 represents model No. 3 with all the accessories, that is, with a complete outfit for the usual chronometric experiments of the laboratory. Model No. 3 was designed not so much for research as for the student practice course, though it can serve for either as far as accuracy is concerned. It differs from No. 2

chiefly in being smaller, much simplified, and in having the especially expensive features eliminated so as to make it possible for a laboratory to have a considerable number of duplicates for general student practice. The base is 25 by 15 cm., while the base of no. 2 is 38 by 21 cm. Other parts are proportionally reduced. The base is made of cast iron, left without planing. The pendulum bracket, which is 15 cm. high, is cast solid and the rests for the knife edges of the pendulum, which in model 2 are separate pieces of hardened steel, are planed out with a properly shaped tool and left without hardening. The pendulum instead of being made from a solid piece, as in model 2, is a composite. A half inch brass rod is selected and a screw thread cut at one end. Pieces of the proper shape for the lower bob and the expansion to hold the axis are soldered on. The upper bob with its detachable part, to be used to secure the slow swing, is screwed on, the fixed part being soldered. The pendulum is then adjusted so as to give a half swing in from 450 to 500  $\sigma$  and a slow swing of about 2000  $\sigma$  or more. The scales for the two swings are made on opposite sides of the same piece of brass; and either side can, of course, be presented by lifting the brass piece out of its guides and putting it in the required way. As these scales are then made from a spark record in the way indicated, theoretically no part needs to be constructed with special accuracy, unless it should develop some irregularity of motion. Among the things needing most care in construction are the axis with its knife edges, the knife edge rests, the hole in the pendulum for the axis, the index armature, the armature of the escapement, and the scales; but for this ordinary good workmanship will probably do.

To make it possible to use the return swing accurately, it is necessary to make the measurement of some known interval longer than the first half swing so that the index will be stopped on the return. With this as a point of reference, the return swing can be read with sufficient accuracy and the proper values can better be assigned to the more thickly crowded divisions at the end.

The difference in the usefulness of models 2 and 3 is represented chiefly by the omission in model 3 of the sliding keys and

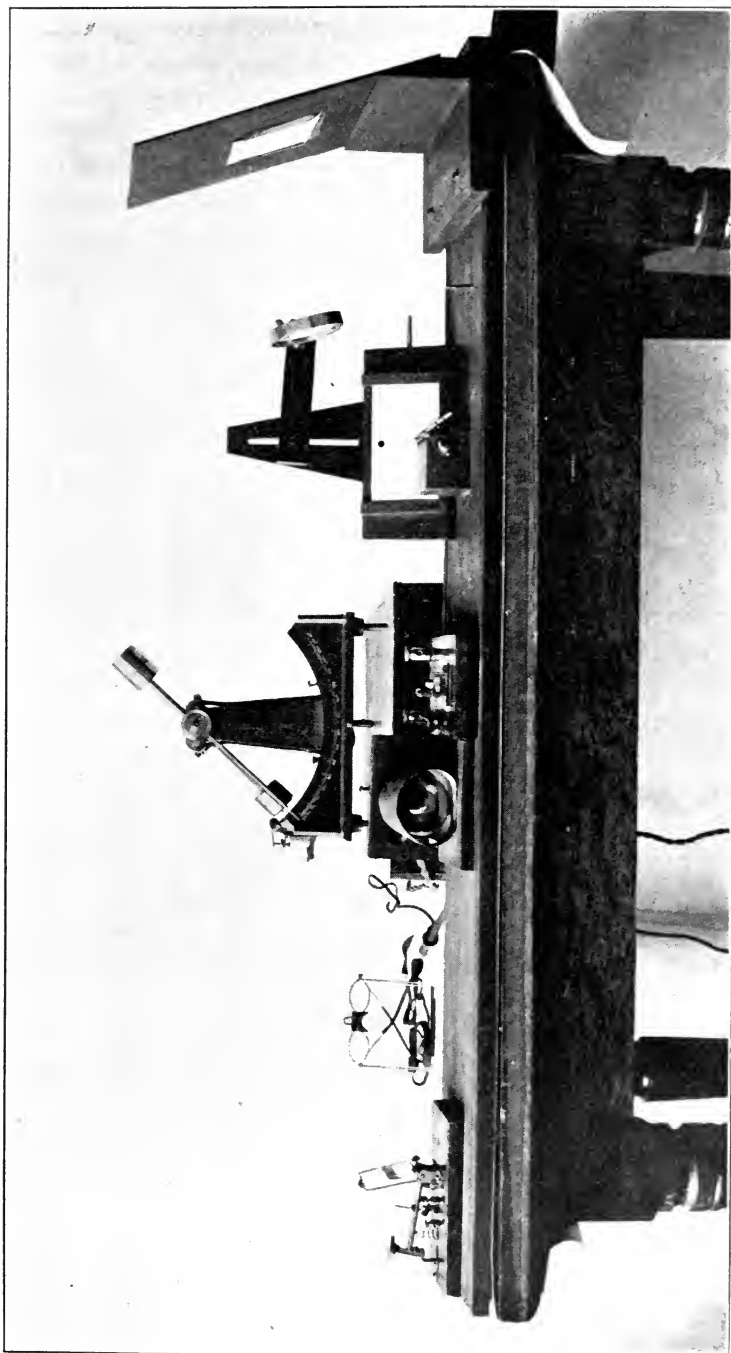


FIG. 3. Model 3 with Accessories.

so of the possibility of using it for securing intervals for tachistoscopic exposure, standardizing other instruments, giving the second signal in complication attention experiments, and for other purposes requiring intervals.

It is, of course, possible to conceive of other types of pendulum chronoscopes, which yet retain the essential features of those here described, especially a fourth, that would have the features of model 3 for reducing cost of construction and at the same time the sliding keys of model 2 for intervals. With such an instrument almost everything could be done that can be done with this line of chronoscopes, and the cost should be much less than for model 2. In size this instrument might be the same as model 2 or between model 2 and model 3, which would perhaps be preferable. The pendulum would need to be somewhat heavier than that in model 3, for the operation of the keys. With one instrument like model 2 and several like model 3, there is, of course, no special occasion for the construction of an instrument of this type and none has been made. But if there were to be but one instrument and model 2 would appear to be too expensive, then the proposed model 4 would seem to meet the requirements.

The accessory apparatus for signals and reactions has been devised to secure accuracy of experimental conditions and at the same time simplicity and convenience. Figure 3 shows the entire outfit. It consists of a board about four feet long, with the various pieces of the accessory apparatus attached. As has been stated, chronoscopes of types 2 and 3 start silently and operate silently till the index is clamped in the reaction. Apparatus for touch and visual stimulation must also operate silently, while the specific function of apparatus for auditory stimulation is to give a regulable noise at the instant of the closure of the circuit for starting the chronoscope. As is well known, reactions for auditory stimulations are shorter than for visual stimulation and for touch stimulation in certain localities, and all reactions are likely to be reduced to auditory reactions, if there is any sound in connection with the different stimulations. It is not safe to trust the reactor's statement as to which stimulation is being followed. He may not be aware that he

is reacting to certain clicks of the chronoscope when he should be reacting to visual stimulation. If the reactor is near the Hipp chronoscope, for example, visual reactions are quite likely to be in reality auditory, due to the sound connected with the beginning of recording by the chronoscope, even though this is so muffled as to be only slightly audible and the reactor himself is not aware that it plays any part in his work.

In the first place, an effort has been made to avoid most of the trouble connected with the setting up of the chronometric apparatus, and especially the trouble connected with changing from one form of reaction to another. The labor of changing is likely to fall on the assistant in the laboratory or on the instructor, rather than on the student; and with a large number of students this introduces a very considerable and unnecessary burden. There is usually not sufficient time for the extended study of the physics of the problem and learning merely to imitate the manipulation of the wiring has probably not sufficient educational value for the time expended. Apparatus for the different forms of stimulation can be ready for use, and in circuit with the chronoscope at the same time, requiring only that a card be slipped in to separate the contacts of the apparatus for touch stimulation in case it is of the break circuit form and is not in use, and for visual exposures of words or figures, that the mirror key be moved from one end of the four foot board to the other and a card holder be substituted for the lens. A card would also need to be slipped between the contacts of the face key, if the break circuit form were used. These changes are slight, however, compared with the labor of setting up separate forms of apparatus for these purposes, especially if correct experimental conditions are to be observed.

While practically all the accessory apparatus is somewhat different from that in use with other instruments, only the device for light stimulation and the exposure of letters, words, figures, colors, etc., is so different that it could not easily be replaced by apparatus of the usual type. For mere light or color stimulation, the mirror key, large lens, and shield with circular opening are required. The circular opening is covered with one or more thicknesses of tissue paper. When the mirror key is

pressed down, at a certain point in its movement, an image of the light will be thrown on the circular opening and the chronoscope will be started at the same instant. This coincidence of the image and the starting of the chronoscope is secured by adjustment. The mirror keys actually in use have an arc at the end of the lever against which a contact spring rests. It is partly insulated so that for a certain distance it can bear against it without starting the pendulum, while this is started when it reaches a certain point in the arc. It is at this point that the image of the light is to be thrown on the circular opening, which occurs without its appearing to come in from below. The purpose of the arrangement, aside from securing contact at a certain point of the movement, is to reduce the sound of the contact so that it will be inaudible to the reactor. If the

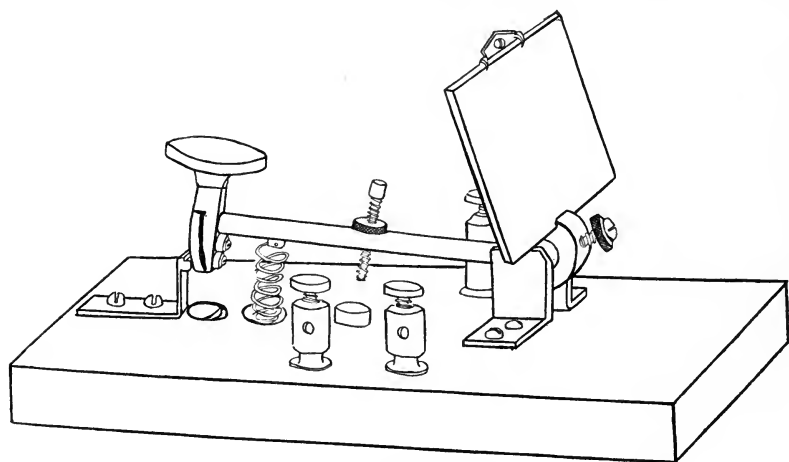


FIG. 4. Mirror Key.

key is well made this is the case, but might not be so after it has become worn. On the whole it seems more convenient than a silent key of the mercury type, as it has to be moved about, even though it will require occasional care. However, the latter might be preferred by others and one could be more certain that it would be sufficiently silent. The source of light for the mirror key shown in Fig. 3 is an electric light placed a little to the left of the middle of the board. This position is not a necessary one and for some reasons a position above the



board would be preferable. Stimulation by different colored light is secured by putting different colored glasses before the source of light. In doing this care must be taken that the illumination of the surroundings be not sufficient to betray the color.

To arrange for the exposure apparatus, the mirror key is transferred to the other end of the board directly under the shield. Two metal posts at either end of the board make this change of position easy and at the same time secure the necessary connection with the chronoscope. The exposure apparatus is shown in Fig. 5. It will be seen that a mirror is placed a

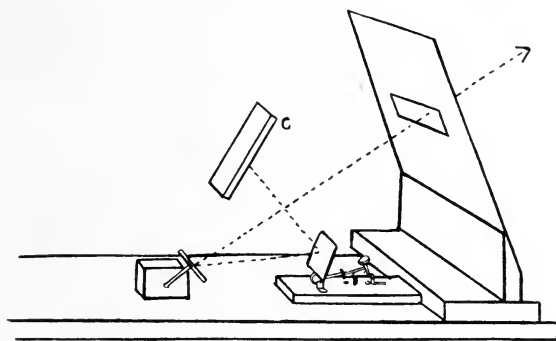


FIG. 5. Exposure Apparatus.

little to the left of the key and that a card holder is substituted for the lens. The subject sees the words or figures in the fixed mirror. When the mirror key is pressed down so as to bring the words or figures into position, it is impossible to tell what they are during the movement, but it becomes possible as soon as the mirror key is arrested, as it is by striking a fixed contact, which has been substituted for the arc contact. The conditions for visual accommodation and fixation seem satisfactory. The words or figures disappear when the mirror key is allowed to move away from contact. If the mirror key were moved by an electric magnet and the current regulated by an interval apparatus, such as that of model 2, this exposure apparatus could be used accurately as a tachistoscope. By some practice, it is possible to press the key down so as to make contact to from one tenth to one twentieth of a second and most of the im-

portant facts of tachistoscopic work can thus be demonstrated, without the attachment of the electro magnet, though, of course, not with satisfactory precision. The position of the card holder is such as to require care in securing its proper illumination. What is seen is seen after double reflection. On the whole good

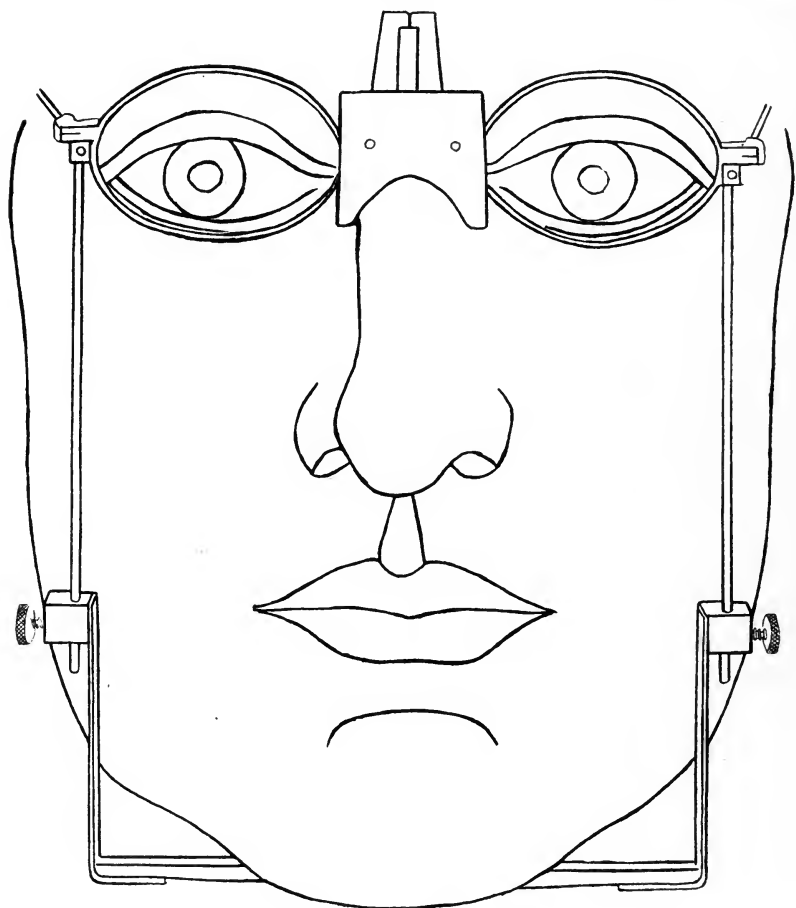


FIG. 6. Face Key.

mirrors of the usual sort, with the silvering on the back will probably in the long run be more convenient, but much better results for a time are obtained by silvering the front surfaces both of the fixed mirror and the mirror of the key, for which glass with an even surface should have been secured. How-

ever, front silvering tarnishes and would have to be repeated every few months to keep the mirrors ready for constant use, but the results are so much better that the trouble is at least worth while for the enthusiast.

The face key, Fig. 6, for showing that there has been the beginning of a movement of the lower jaw, as in beginning to speak a word, is probably next in the degree of difference from apparatus ordinarily in use. The recording of this reaction accurately is difficult and a little practice is necessary to secure good results. As has been stated, the key records merely the time of opening the mouth. Some subjects will open the mouth before they are ready to react. If this occurs, they must be trained to wait until they begin to express a word by this movement. The key has been made only in the break circuit form, but can, of course, also be made in the make circuit form. In the pendulum chronoscopes here described, all the circuits are make circuits, which in case break circuit keys are used, requires that they be used as short circuits. In the case of the break circuit keys of the interval apparatus, they must also sometimes be used as short circuits to secure a make circuit connection in the main line. The break circuit face key must therefore be used as a short circuit, which involves no great difficulties, if there is sufficient power.

In constructing the face key spectacle rims are taken and arranged as shown in the cut. The nose piece contains the springs for keeping the contacts together. The key must stay rather firmly in place, so it may respond accurately to the beginnings of movements. For this purpose the key is tied with a band behind the head. Rather heavy frames, or light frames reenforced by copper wire, are best for carrying sufficient current. With good construction, the durability ought not to differ greatly from that of any other pair of spectacle frames. The subject feels more at ease and the hygienic features are more convenient, if not better, than they are in the case of keys held between the teeth. The touch stimulation key, represented in Fig. 7, does not differ in any important respect from the usual. The rate of separation of the contacts, however, is not less but greater than the rate of motion of the point touching the skin.

The sounder, represented in Fig. 8, for sound stimulation is somewhat different from the usual dropping ball apparatus. It consists of a small hammer held down against the surface it strikes by a spring. The contact of the hammer with this surface starts the chronoscope. The spring holding the hammer

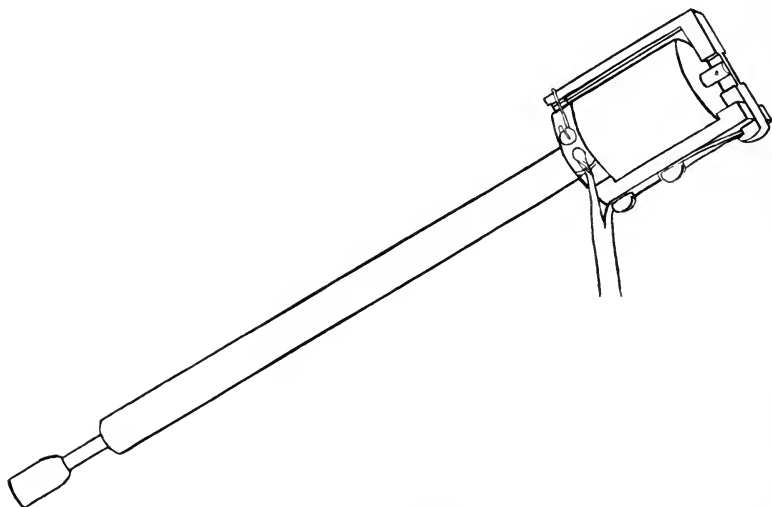


FIG. 7. Touch Key.

down should be under sufficient tension to prevent any rebound. The scale enables the experimenter to use the same or different intensities of sound stimulation, as he may wish. An attempt to arrange the scale for equal audible or equal physical units of intensity has not been made. In this respect the dropping ball apparatus has the advantage of having the ball drop with kinetic energy proportional to the height of the fall, though it is not certain that the complex sound caused by the ball striking the bridge and the bridge in turn the base is precisely proportional to the kinetic energy. For accurate work, the intensity both audible and physical of the sound from the falling ball apparatus would probably also need to be determined. The sound from the falling ball apparatus is dull, while that from the sounder here described is relatively sharp. It is proposed chiefly because of its convenience, as it furnishes accurate conditions in a small compass, except for experiments upon the

effects of varying intensities, for which it would require further graduation.

With regard to the accuracy of this type of chronoscope, the writer has made several tests at different times. Some results are reported in the article describing model no. 1. Instead of adding to these, some results kindly lent by Professor W. D. Scott, of Northwestern University, will be summarized. The

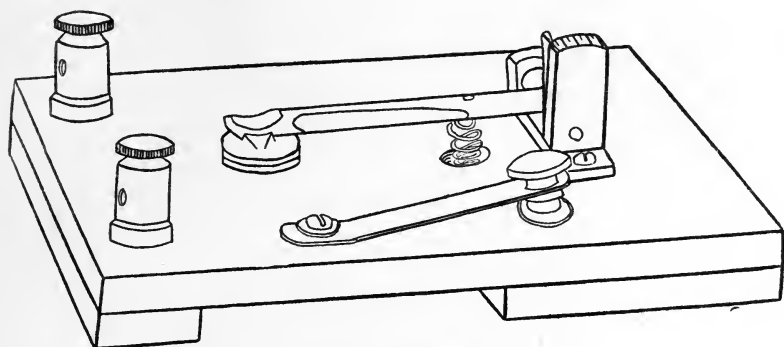


FIG. 8. Sounder.

tests were made on a chronoscope of model 2, constructed by Stoelting & Co., and were executed by Mr. Gordon Fulcher. The account of the tests points out that variations in the record may be produced by varying the current either in the escape-ment or the index coils; and the records giving the absolute time are obtained only with a certain adjustment of the escape-ment and index.

The testing apparatus consisted of two falling weights, one striking a lever starting the chronoscope, the other striking a lever and arresting the index. The possible errors of the testing apparatus appear to have been very small.

Under circumstances most favorable to the chronoscope, he obtained readings for intervals of  $200\sigma$  and  $100\sigma$  'as closely identical as could be determined by the eye.' His general conclusion is that the chronoscope could be made to read to  $1/5\sigma$ . By special care in construction or by adding a certain constant to the averages, this adjustment of the chronoscope which gives the best results could be made permanent. In adjusting it so as to

secure absolute records directly, however, somewhat less uniform results were obtained. For an interval of  $200\sigma$  he obtained records of 199.9, 200, 199.8, 200.1 and  $200\sigma$  and for  $100\sigma$  he obtained 100.5, 100, 100.1 and  $100.5\sigma$ .

In dealing with records as small as a fraction of  $\sigma$ , the utmost precaution must be taken to avoid any disturbance. For example, the slight jar given the chronoscope by having the key of the testing apparatus on the same table with it had a determinable effect, when it was brought down sharply, and the accuracy attained in practice will doubtless be somewhat less than in these tests in which the utmost care was taken.

## MENTAL PROCESSES AND CONCOMITANT GALVANOMETRIC CHANGES.<sup>1</sup>

BY DANIEL STARCH, PH.D.

*Problem.*—The purpose of this investigation was to observe and to record the changes in the resistance of the body to a weak electric current during different types of mental states and processes. The specific problems were to determine: (1) Whether all or only some mental processes are accompanied by changes in electrical resistance, (2) whether different types of mental processes are accompanied by characteristically different variations in resistance, and (3) whether the degree of intensity or vividness of a given process is accompanied by a corresponding amount of change in resistance.<sup>2</sup>

*Apparatus.*—The apparatus consisted of two electrodes, a Leeds and Northrop mirror galvanometer, and a chloride storage cell. Considerable difficulty was experienced in obtaining satisfactory electrodes. In a large number of preliminary experiments electrodes similar to those employed by Tarchanoff<sup>3</sup> were used, but they were discovered to be polarizable and unreliable and therefore had to be discarded. Those finally found to be satisfactory and used in all the experiments described in this paper were the Ostwald<sup>4</sup> calomel-normal electrodes. Thistle tubes, 12 cm. long and 3 cm. in diameter at the top and 2 cm. at the bottom, served as the vessels into which the following contents were placed: A piece of platinum wire fused into the bottom of the tube; mercury to the height of 1 cm.; above the mercury a mixture (Brei) of calomel and mercury, 1.5 cm.

<sup>1</sup> From the Psychological Laboratory of Harvard University.

<sup>2</sup> This phenomenon, frequently called the 'psycho-galvanic reflex' was discovered in 1888 by Fere and further studied by Tarchanoff in 1890. A brief résumé of contributions on this subject can be found in the article by Peterson and Jung in *Brain*, 1907, Vol. 30, pp. 153-218.

<sup>3</sup> Tarchanoff, 'Ueber die galvanischen Erscheinungen in der Haute, etc.,' *Archiv f. d. ges. Physiologie*, 1890, Vol. 46, pp. 46-55.

<sup>4</sup> Ostwald-Luther, *Physiko-Chemische Messungen*, Zweite Auflage, Seite 381.

high; calomel shaken with mercury, 2 cm. high; a porcelain sieve; cotton (supported by the sieve) saturated with a 7.45 per cent. solution of KCl. The cotton filled the vessels to the top and made contact with the skin to which they were applied. To secure uniform cleanliness, the upper layer of cotton was frequently removed and fresh cotton substituted. These electrodes were mounted in convenient wooden frames which kept them upright and also served as means of attaching them. The upper part of the frame consisted of a light horizontal board through which the electrodes projected sufficiently to make good contact with the hands or other area of the skin placed upon them. The hands were held in place by wide, flexible leather straps which were fastened to the frames. The electrodes were put in circuit with the cell and resistance coils aggregating 8,800 ohms.

In order to have the mirror as free from jars as possible, the galvanometer was placed upon a support fastened to the wall of the building. The reading telescope was so set upon a table that the scale was at a distance of 1 m. from the mirror. In this manner the scale was reflected into the telescope, and a very slight turn of the mirror would therefore reflect different parts of the scale. The sensitiveness of the galvanometer was such that a change of deflection of 1 cm. on the scale meant a change of .001 milliampere in the galvanometer. The latter was shunted by an adjustable resistance so as to regulate the amount of current that should go through it. Before each experiment the two electrodes were connected by moist filter-paper instead of the subject's hands, and the slide of the resistance was so adjusted that the deflection of the mirror was 10 cm., from 33, the zero point, to 43. Usually very little adjustment was necessary, because the cell was fairly constant, which insured considerable uniformity in the current. The cell was always connected with two Edison-Lalande cells except during the experiments.

*Procedure.*—The subject was comfortably seated on a chair beside a table and required to be as passive as possible by avoiding all activity except that connected with the different forms of stimuli. Usually the subject had his eyes closed



except when they were needed for the experiment, or else confined his view to some previously determined point. The palms of the hands were placed upon the electrodes and held in place by the straps passing over the backs of the hands. The electrodes were placed wherever it was most convenient for the subject, usually either on a support in front of him just above the knees, or on chairs beside him. In order to give uniformity to the condition of the skin of the various subjects, every one was required to wash his hands before the experiment.

The readings of the galvanometer were obtained by observing the scale as seen through the telescope and recording the extremes of the oscillations of the mirror. The average of two successive extremes was regarded as the determination of the deflection of the mirror from the zero point. The following is an illustration of the records thus obtained.

		Av.
33.0		
33.9	33.8	33.85
34.4	53.6	34.0
34.3	34.0	34.15
34.1	34.0	34.05
34.2	34.0	34.1
34.2	34.0	34.1
34.3	33.9	34.1
34.3	34.1	34.2
* 34.3	34.3	34.3
35.2	34.7	34.95
34.8	34.6	34.7
35.4	34.4	34.9 etc.

33.0 is the zero point of the mirror before the subject's hands were attached to the electrodes. As soon as the hands were attached and the circuit completed the mirror turned to 33.9 and immediately back to 33.8, then to 34.4 and back to 33.6, etc. At the point indicated by the asterisk a stimulus was applied, and consequently the deflections were greater, which means a decrease in the resistance of the body.

Ten subjects were employed in the experiments: *B, D, F, H, N, P, R, Rg, T* and *V*. All were members of the psychological laboratory of Harvard or Radcliffe, *D* and *P* being women. Not more than one record was obtained from a given person on one day.

*Records of Normal Passivity.*—The object of the first series of experiments was to obtain records from normally passive subjects. Such records would at once reveal whether the resistance would change when the subject was at rest, and would also furnish standard curves with which to compare all other records obtained when stimuli were applied.

The procedure was simple. The subject was asked to take a comfortable position on the chair and told to be as completely passive as possible. His hands were then connected with the electrodes and readings were taken for twenty minutes. Immediately at the completion of the record the subject wrote a careful introspective account of the period. One record was obtained from each subject.

The results can best be represented by curves, Fig. 1. The points in the curves are determined by the averages of the successive pairs of readings as explained above. The numbers at the left of the figure represent the scale in centimeters, 33.0 being the zero point. The letters at the beginning and end of the curves refer to the subjects. A rise in the curve means greater and a drop means less deflection of the mirror from the zero point. Or in terms of resistance a rise means a decrease and a drop an increase.

From these records it is plain that the typical passive curve runs along by small zigzags on the same general level, indicating that the resistance is practically constant under normal passive conditions. Seven of the ten curves agree on this point. The other three, *F*, *P*, and *Y*, show very distinct rises about the middle of the curves. These deviations are, however, explained by marked disturbances which occurred at the points indicated. In the case of *P* some one unexpectedly entered the room at the point marked  $\alpha$ . This aroused a strong emotional state especially because the subject could not turn to see who it was. The record was therefore terminated at the end of fourteen minutes. At  $\alpha$  in the curve of *Y* there was a loud disturbing noise in the adjoining room and a few seconds later the subject moved one of his hands. *F* seemed to be somewhat restless throughout the entire period. At  $\alpha$ 1 he opened his eyes and turned his head to look at the tower clock on a

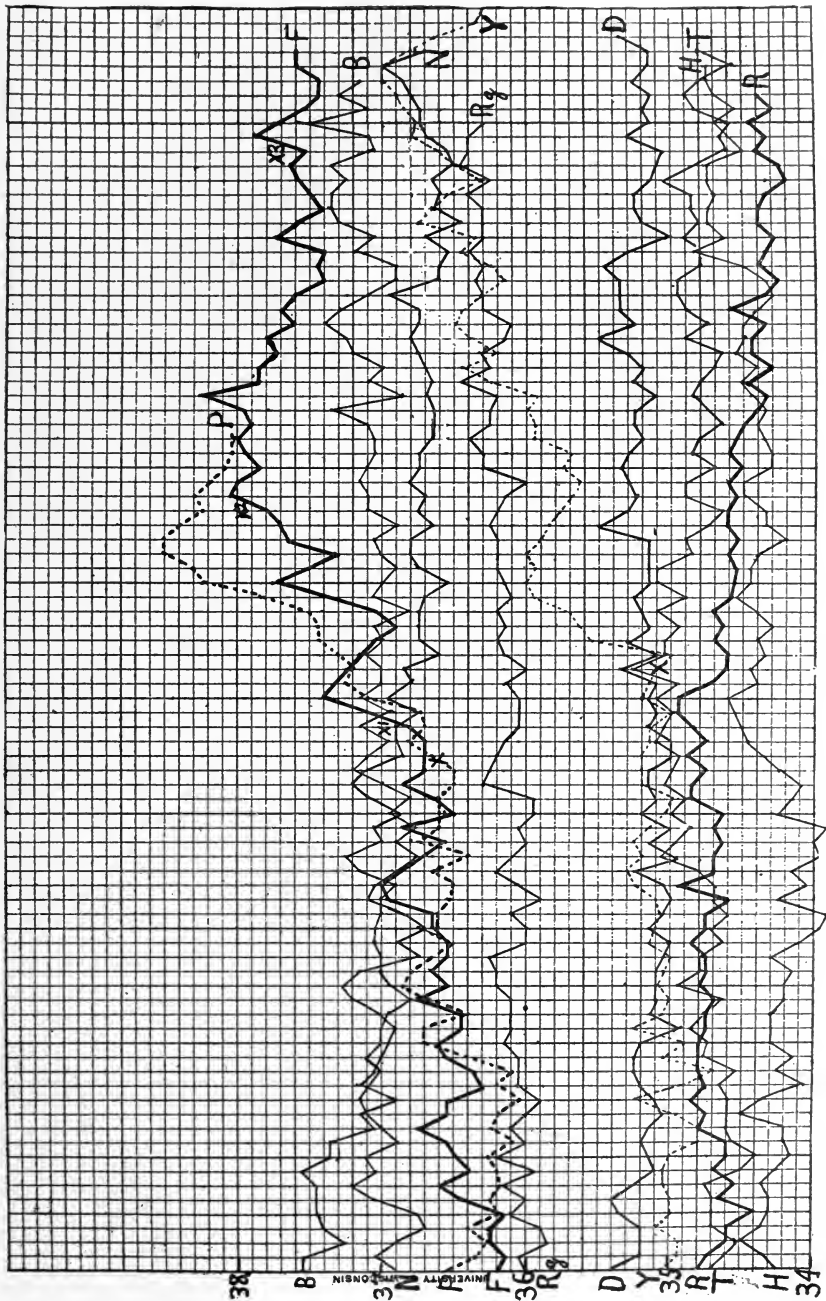


FIG. I.

neighboring building. When it occurred to him that he should not have done this and that it was disturbing to the experiment, the whole situation seemed rather humorous to him. At  $\approx 2$  he swallowed and took a deep breath, and at  $\approx 3$  he again took a deep breath. Later experiments will show further that similar deviations in the curves are due to such disturbances. The smaller fluctuations of the curves are probably due to less marked mental changes.

The individual differences are indicated by the fact that the curves are on different levels. Apparently some individuals offer more resistance than others.

*Muscular Work.* — The aim in this series of experiments was to obtain records of subjects doing muscular work. The type of work desirable for this purpose was voluntary muscular work involving as little mental activity as possible. The task selected was the repeated lifting of the right knee at a rhythm chosen by each subject. The height of the lifting was about five inches, which was determined by a crossbar fastened five inches above the knee. This work was continued by each subject as long as it comfortably could be without causing too much fatigue.

The records were taken in the following way. The electrodes having been attached, the subject was asked to be passive during the first two minutes after which at the signal 'Now,' he began to lift the knee as directed before the experiment. When the work was stopped the subject again took a passive attitude and readings were continued for several minutes longer, varying in the different records from three to six minutes. Immediately after the experiment the subject wrote an introspective account.

The results are represented by the curves in Fig. 2. The beginning and the end of the work periods are indicated by short heavy lines across the curves. Two of the subjects were asked to stop the lifting of the knee, *T* at the end of ten minutes and *D* at the end of eight minutes. The others were fatigued more quickly, *B* continuing the task for only two minutes. In no case, however, was the fatigue more than moderate.

The results indicate without exception a rise in the curve, *i. e.*, a decrease in resistance as soon as the work was begun.

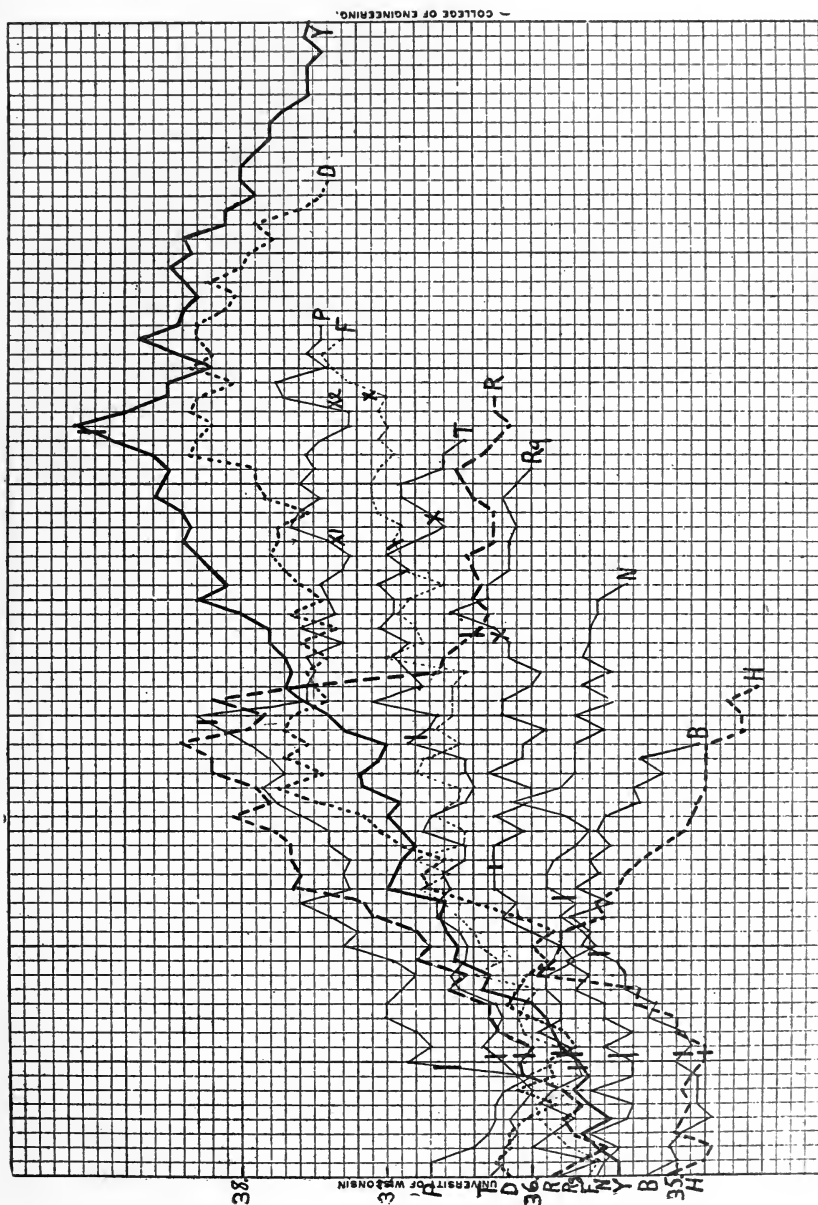


FIG. 2.

*T*, the heavy continuous line, is a typical curve, rising gradually during the period of activity and dropping slowly after the cessation of work. Some curves rise very considerably above the initial passive period, for example *T*, *D* and *R*. Two curves, *N* and *Rg*, show only a small rise. In some cases the rise is quite rapid, in others rather slow. All but three curves, *N*, *T* and *F*, show a drop in the final passive period.

The evident conclusion of these experiments is that the bodily resistance decreases during muscular work. The introspective statement of the subjects further corroborate the observation made in connection with the preceding series of experiments that the resistance is decreased by emotional excitement. For example, in the curve of *P* the subject took a deep breath at  $\alpha 1$ , and at  $\alpha 2$  a loud noise occurred in the adjoining room. At  $\alpha$  in curve *T* the subject heard someone speak which vividly recalled a certain discussion. At  $\alpha$  in curve *F* the subject remarked that his right hand 'itches like sin.' At  $\alpha$  in curve *Rg* the subject took a deep breath.

*Mental Work.* — In contrast with the experiments just described another series was undertaken to find the effect of work distinctly mental and accompanied by a minimum of muscular activity. The task selected was the effort of visual attention to objects in the indirect field. A small cross drawn on a piece of paper served as the fixation point. Twelve centimeters to the right of this was written a group of six capital letters, each one centimeter high. The task consisted in focusing the eyes upon the cross and in directing the attention at the same time to the group of letters to make out what they were. Twelve centimeters was far enough from the point of regard to make the task sufficiently difficult. No subject succeeded in reading all the letters during the course of the experiment. The sheet of paper bearing the letters was placed in a convenient position upon the table at which the subject sat. The letters were covered with another sheet of paper until they were to be used.

In taking the records the subject was asked to be as passive as possible during the first two minutes. At the signal 'Now' he opened his eyes and fixated the cross. The letters were then uncovered. The subject kept up his task of attempting to read

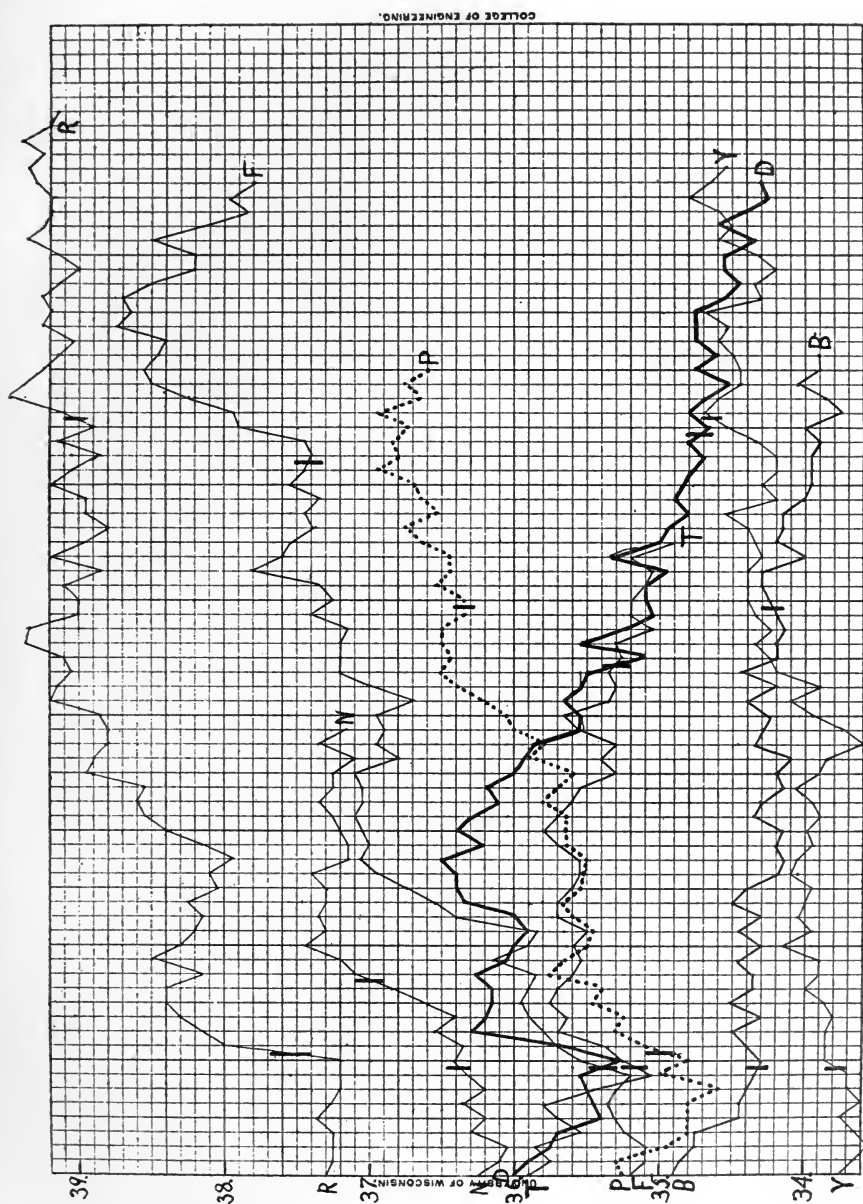


FIG. 3.

them as long as he comfortably could. This was then followed by a passive period of four minutes.

Eight records were obtained, shown in Fig. 3. Two subjects did not succeed well in controlling the eyes and so their records were discarded. All the curves, with one exception, rise during the performance of the mental task. The curve of *B* gradually drops from the beginning. This was probably due to the subsidence of an emotional state experienced just before the experiment.

Comparing these records with the curves of muscular work, we notice the significant difference that the former do not rise as high as the latter. Quiet mental work, although performed with considerable effort, is apparently accompanied by a rather small galvanometric change. *X*, whose curve of muscular work rises very considerably, shows a very much smaller rise in the curve of the mental work, although, as he states in the introspective account, he made a very conscious effort to perform the visual task. The conspicuous rise in the latter half of his curve may be due according to his introspection partly to the discomfort from not being able to "blow his nose." Similarly the deflection in *P*'s curve may be due principally to the feeling of fatigue. This subject says: "The strain was very noticeable especially toward the end of the period . . . which caused me to be quite uneasy and nervous." *R*, whose curve also shows a higher deflection than the average, states in his introspection: "Fixation of the spot was not difficult but the attendant effort to hold the attention on the letters was very fatiguing. It involved an eye control which produced a conscious tension in both legs and arms."

The conclusion from these records, therefore, seems to be that mental activity is accompanied by a noticeable though milder galvanometric reaction than muscular activity.

*Automatic Mental and Muscular Activity.* — The next problem was to compare automatic muscular and mental types of activity with each other and with the voluntary types of the two preceding sections. For this purpose the repetition of the multiplication tables, either quietly or aloud, was selected. This is of course only partly automatic activity but it served



the purpose well because it could be accompanied, when desired, by equally automatic muscular activity.

The course of a record was as follows: two minutes passive, two minutes repeating the 3's quietly (or aloud), two minutes passive, two minutes repeating the 4's aloud (or quietly), two minutes passive.

The results are tabulated in Table I. The figures express in centimeters the maximum deflection during a given period of activity over the last reading of the passive period preceding it. Thus *D* shows no increase in deflection over the first passive period when repeating the 3's quietly, but the loud repetition of the 4's is accompanied by an increase in deflection of 1.45 cm. over the second passive period.

TABLE I.

	3's Quietly.	4's Aloud.
<i>D</i>	.00 cm.	1.45 cm.
<i>H</i>	.05	.20
<i>N</i>	.15	.55
<i>F</i>	.45	.45
<i>T</i>	.20	.60
<i>Y</i>	.15	.75
Av.	.17	.67
	3's Aloud.	4's Aloud.
<i>P</i>	.85 cm.	.65 cm.
<i>B</i>	.45	.45
Av.	.65	.55

The interesting fact brought out in this table is that habitual mental activity is accompanied by very little change in the galvanometric readings, only .17 cm. on the average, while similar activity accompanied by automatic muscular activity produces a distinct rise, on the average .67 cm. To show that this difference was not due to the particular table repeated nor to the succession of repeating quietly in the first period and aloud in the second period, the last two subjects repeated both 3's and 4's aloud. In each case a distinct rise is present, .65 cm. and .55 cm. Apparently the difference was due to the additional exercise of the muscles involved in speaking.

In comparison with the preceding series of experiments we

notice that automatic mental activity produces less change than voluntary mental activity (visual attention). The maximum increase in deflection in the latter was nearly 3.00 cm. (*R*'s record), while in the former it was only .45 cm. The voluntary muscular work of lifting the knee is entirely different and exercises much larger muscles than does the loud repetition of multiplication tables, so that the two can not well be compared. It seems, however, that voluntary muscular activity produces greater galvanometric changes than automatic muscular activity.

*Emotional States: Stimuli Unexpected.*—During the experiments thus far described it was frequently observed that emotional experiences were particularly effective in producing deflections. This was suggested again and again by the disturbing incidents. The remaining experiments were therefore devoted to the study of various types of emotional states.

In the first group of experiments the stimuli were unexpected. The subject was told to assume a passive attitude and that a record of fourteen minutes would be taken. During the course of the record an electric bell concealed under the table was rung three times, each time for five seconds. The ringing occurred at the end of the second, sixth and tenth minutes. These points are indicated in the curves by short, heavy cross lines, Fig. 4.

Almost without exception each ringing of the bell is followed by a distinct rise of the curve. *Y*'s curve is a typical illustration. In connection with this record the following quotation from his introspective account is of interest.

"I was much startled when the bell first sounded and for a moment experienced vivid emotion. I suspect that my heart beat was markedly influenced but I did not note it particularly. I noticed that my rate of breathing increased when the bell sounded the second time. The second and third ringings did not surprise me but they stimulated me strongly and certainly produced physiological changes."

*Rg*, whose curve indicates smaller deflections than the record of any other subject, says the following in his introspection.

"I was conscious of very little, if any, emotional reaction from the bell at any time. There may have been a momentary start

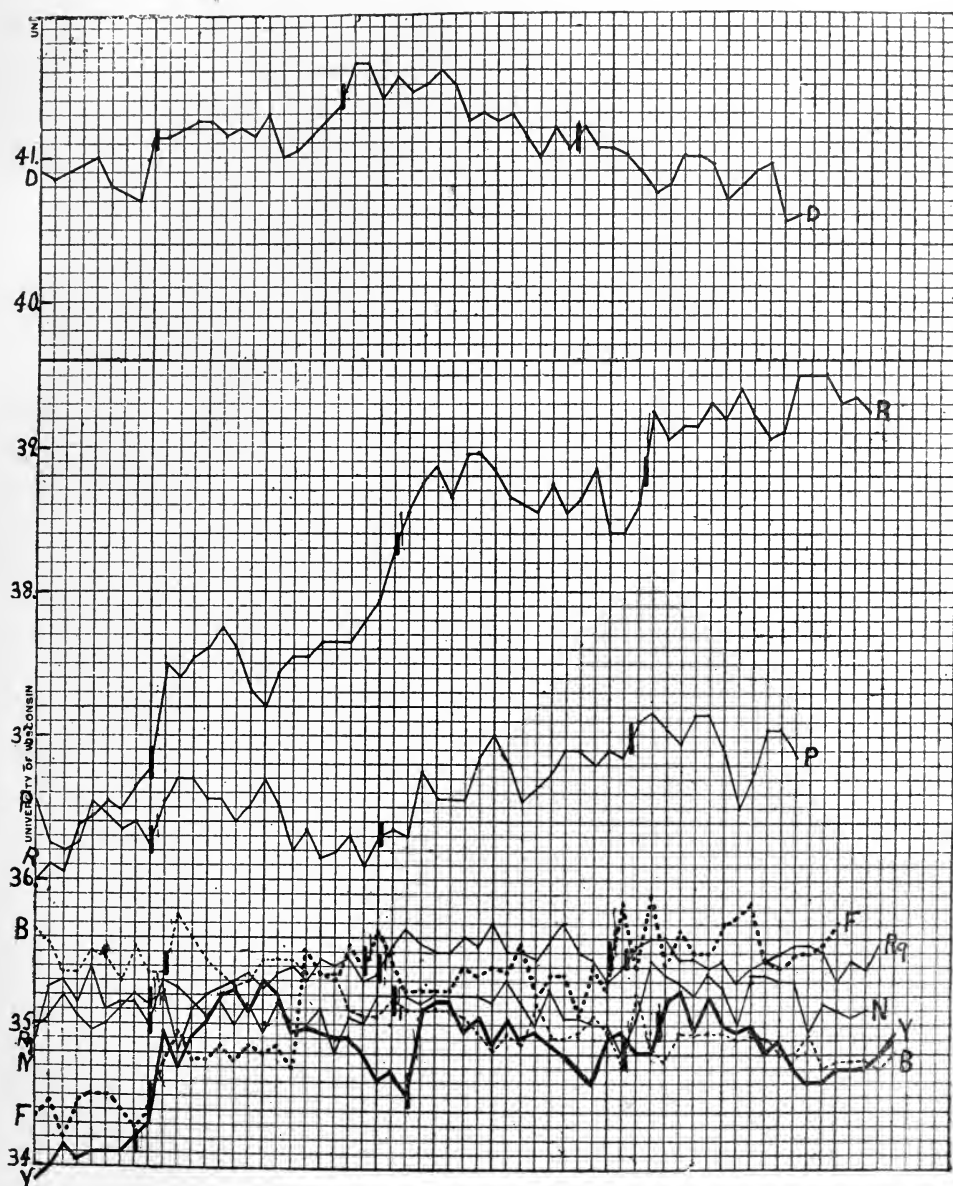


FIG. 4.

but it was immediately inhibited. I should place the reaction as very weak. I was not conscious of any change in heart beat or in breathing. I am accustomed to a large house with an electric bell system and annunciator, and therefore am used to bells at all hours, day and night."

In order to determine more accurately whether the intensity of the emotion is accompanied by a corresponding amount of deflection, the subjects were required to estimate as accurately as possible in their introspective accounts, the intensity of each emotional experience resulting from the ringing of the bell. The following arbitrary scale was adopted: no emotion, weak, medium, strong. All the records are tabulated in Table II., showing the estimated strength of the emotion and the corresponding deflection above the last reading preceding the ringing. Thus in curve *Z* the last reading before the first bell was 4.2 and the maximum reading following the ringing was 35.3. The difference, 1.1, is the figure tabulated. The figures show a fairly close relation between the amount of deflection and the intensity of the emotion.

TABLE II.

	First Bell.	Second Bell.	Third Bell.
<i>B</i>	Medium, .40	Weak, .15	Weak, .15
<i>D</i>	Weak, .10	Medium, .30	Weak, .15
<i>F</i>	Strong, .60	Strong, .25	Medium, .65
<i>N</i>	Strong, .30	Weak, .00	Strong, .65
<i>P</i>	Weak, .45	Weak, .45	Weak, .25
<i>R</i>	Weak, .85	Medium, .65	Strong, .80
<i>Rg</i>	Weak, .10	Weak, .30	Weak, .20
<i>Y</i>	Strong, 1.10	Medium, .70	Medium, .45
Av.	.49	.35	.41

Average of all described as weak, .25.

Average of all described as medium, .52.

Average of all described as strong, .62.

*Emotional States: Stimuli Expected.*—The next series of records was obtained in a similar manner with the exception that before each bell two warning signals were given. The subjects were told that the signal 'Ready' would be given thirty seconds, and the signal 'Now' two seconds, before the ringing of the bell. The signals were given so that the ringing would occur at the end of the second, sixth and tenth minutes.

Here, however, another variation was introduced. The bell was rung only at the end of the second and sixth minutes. The third time the signals were given as before but the bell was not rung. This, of course, was unknown to the subjects. The aim was to see what the effect of expectation, without the occurrence of the stimulus, would be.

The results are presented in Table III. which is compiled in the same manner as Table II. No records were obtained from four subjects.

TABLE III.

	First Bell.	Second Bell.	No Bell.
<i>B</i>	Strong, .40	Weak, .00	Strong, 1.80
<i>D</i>	Medium, .70	Weak, .20	Medium, .50
<i>P</i>	Strong, .45	Medium, 1.85	Weak, .85
<i>R</i>	Medium, .65	Strong, .25	Weak, .00
<i>W. G. H.</i>	Weak, .25	Weak, .10	Weak, .30
<i>Y</i>	Weak, .05	Weak, .25	Weak, .50
Av.	.42	.44	.66

Averages of all described as, weak, .25 ; medium, .92 ; strong, .72.

The average deflections after the first and second ringings preceded by the signals are practically the same as in the foregoing records where the subjects had no inkling of what was coming, .42 cm. and .44 as compared with .49, .35 and .41. The reason for this probably lies in the fact that the warning signals affected the different individuals in quite opposite ways. In some, the emotional excitement seemed to be heightened through the signals so that in several instances a deflection occurred immediately after the first signal. In others, the signals brought about a calm preparation for the sound. As an illustration of the former the following quotation from *P*'s introspection may be cited.

"Just after the signal 'Ready' I began to be conscious of my breathing. It seemed more hurried. I had the feeling as if I needed to take a long breath but couldn't. At the same time I became conscious of the beating of my heart and of a tension in the muscles. These symptoms became more marked, reaching a crisis just after the signal 'Now.' When the bell rang I 'started' very slightly but I had the consciousness of a sudden and marked change. The emotion was strong."

As an illustration of the second class of subjects, *Y* may be quoted.

"The bell caused no disturbance. There was a relaxation immediately for I had been expecting it on account of the signals. If any emotion was aroused by the bell it was one of mild satisfaction and amusement. I do not know that either my heart beat or my respiration was influenced. The influence of the bell was even less the second time. I was not disturbed by its failure to come the third time but I was again amused."

The third stimulus of giving the signals without the actual ringing was followed on the average by a greater deflection than the actual ringing, .66 cm. as compared with .42 and .44. The emotional excitement caused by the omission of the bell was in most subjects quite vivid and usually amusing. *B* wrote the following statement:

"The 'Now' signal, preceding the omission of the bell, resulted in a considerable alertness and a mental bracing against the coming sound. This resulted, I think, in deeper and longer breathing. When the sound did not come, tension grew rapidly greater until I realized the situation, and the joke of it produced considerable amusement, which, however, I tried to suppress. It went and came intermittently several times. The breathing became shallow and quick. Then relaxation and drowsiness returned."

These introspective statements correspond on the whole very closely with the extent of the deflections. *P*, who reports an intense emotional experience, shows very large deflections, and *B*'s description of the failure of the bell also correlates with a large rise, 1.80 cm. On the other hand *Y*, who was little disturbed by the stimuli, shows very small galvanometric changes. Too much significance, however, should not be attached to the figures because of the rather wide distribution. For example, those described as 'medium' range from .50 to 1.85. This latter reading accounts for the fact that the average of the 'mediums' is larger than the average of the 'strongs.'

*Emotional States: Unpleasant Stimuli.* — To determine the effect of unpleasant stimuli, records fourteen minutes in length were obtained during which three stimuli were applied by an

assistant, each one for fifteen seconds, at the beginning of the third, seventh and eleventh minutes respectively. The first stimulus was an unpleasant odor, the second was a spring clamp attached to the lobule of the left ear, and the third was a succession of needle pricks on the back of the left wrist.

The deflections produced by these stimuli are given in Table IV.

TABLE IV.

	Odor.	Clamp.	Needle.
<i>B</i>	Weak, .05	Medium, .30	Weak, .00
<i>F</i>	Medium, .60	Medium, .55	Weak, .25
<i>R</i>	Weak, .45	Medium, .40	Medium, .20
<i>Rg</i>	Medium, .35	Medium, .50	Medium, .55
<i>T</i>	Medium, .50	Medium, .45	Strong, .45
<i>Y</i>	Medium, .55	Strong, .85	Strong, .60
<i>D</i>	.40 <sup>1</sup>	.25	1.05
<i>P</i>	.40	.40	.60
Av.	.41	.46	.46

The stimuli used were of course not equally unpleasant to all the subjects. The odor was quite disagreeable to some and practically indifferent to others. Nevertheless, the subjective gradations of unpleasantness agree very closely with the readings. The average deflection for 'weak' is the smallest, and for 'strong' the largest.

*Reading Informative and Humorous Passages.*—Four records were obtained to compare the effects of reading two different kinds of materials. One was a description of political events, the other was a short humorous story, each containing about seven hundred words. Both were read quietly by each subject during the course of a record. One was started at the beginning of the third minute and the second was started at the beginning of the eighth minute in the record. The interval between the two readings varied from one half to four minutes, depending upon the rapidity of reading.

The results in Table V. indicate that both readings were accompanied by deflections. The 'humorous' reading, however, produced larger deflections than the 'informative,' .42 cm. and .29 respectively. The explanation probably lies in the fact

<sup>1</sup> By mistake *D* and *P* failed to write introspective accounts.

Averages of all described as, weak .19, medium .45, strong .63.

that the former reading was accompanied by a stronger emotional tone.

TABLE V.

	Informative.	Humorous.
<i>R</i>	.20	.25
<i>Rg</i>	.25	.40
<i>P</i>	.35	.50
<i>Y</i>	.35	.55
Av.	.29	.42

*Summary of Results.* — 1. During undisturbed passivity practically no galvanometric changes occurred.

2. All the different types of mental processes produced by the various stimuli were accompanied, without exception, by galvanometric changes.

3. Some mental processes were characterized by very much larger deflections than others. Emotional processes and muscular activity produced the widest deflections, while habitual mental activity and the process of visual attention produced the least deflections. Quiet mental activity even when involving considerable effort produced small galvanometric changes. It therefore seems to follow that those types of mental processes which are accompanied by the most distinct and widespread physiological changes produce the largest deflections, such as, muscular activity in lifting the knee; strong emotions accompanied by a 'start' or 'shock,' halting and quickening of respiration, muscular tension, etc.

4. The degree of intensity of emotional experiences corresponds very closely with the amount of deflection. Combining Tables II., III. and IV., the average deflection of all emotions rated as 'weak' is .23 cm., of all rated as 'medium' .63, and of all rated as 'strong' .66.

This investigation has dealt primarily with the determination of amounts and types of galvanometric changes. Whatever the ultimate cause of these changes may be, whether it be in the resistance of the skin or in the variation of other functions, the results here set forth will not necessarily disagree with either explanation.



## THE DEVELOPMENT OF RIGHT-HANDEDNESS IN A NORMAL INFANT.

BY HELEN THOMPSON WOOLLEY.

In making some tests on color vision during the seventh month of a normal infant's life, the method used was that of presenting two discs laid side by side directly in front of the infant, and recording the one selected. The hand used in picking up the disc, and the position of the disc chosen were also recorded. The same data were kept in making a similar series of tests with a wooden square and circle. In the color tests, the discs were all placed within easy reaching distance; but in the series with the square and circle, the distance was varied from three to ten inches, and the effect on the hand used recorded. The color tests were begun at the beginning, and those with the square and circle at the middle of, the seventh month. Both series were discontinued at the close of the seventh month, for reasons given in reporting on the color tests. The number of color tests included in this report is greater than that in the tables on color choice, because some series which were invalidated for color for one reason or another, were still significant with reference to the use of the hands.

The few instances in which the child picked up one object in each hand were discarded. For the color series, where the distance remained constant, the hands were used as follows: right hand, 206 times; left hand, 194 times; both hands, 68 times. The difference in the use of the right and left hands for the entire series is not great enough to be significant, but in reviewing the chronological order, it is interesting to note that the days on which the use of the left hand predominated, were all, with one slight exception, during the first week of the month. In fact for the first week, the left hand was used more frequently than the right, but after that, the right hand predominated steadily.

The experiment of testing the effect of the distance of the

object on the use of the hands in grasping, was first tried by Professor Baldwin with an infant in its ninth month. His observation that reaching called out the use of the right hand almost exclusively, at a time when little if any distinction in the use of the hands could be detected in activities not requiring effort, was easily verified in the case of this infant in the seventh month. A series of informal tests showed the preference for the right hand in reaching, so uniformly that formal experiments on the point were scarcely necessary. However, the following series of 70 tests was carried out. The distance of the square and circle was measured from the center of the object to the child's body.

Inches.	Right.	Left.	Both.
3	7	13	0
5	4	8	2
7	13	7	0
8	10	0	0
10	6	0	0

Seven inches is the distance which just required reaching, while ten inches required so much effort that the child was loath to make it. A larger series of results would doubtless have produced some instances of the use of the left hand in reaching for objects more than seven inches away, but I feel confident, in view of the uniform results of informal tests, that a longer series would not have changed the trend of the table. During the same period I found that she also used the right hand for catching a swinging object, and for picking up an orange, a very difficult accomplishment at that age.

After the beginning of the eighth month I have no experimental material, but I have notes on the normal use of the hands. During the seventh month, the period of the tests, my general observations showed no signs of right- or left-handedness. I should have said that none existed, were it not for the experimental evidence. The same holds true of the eighth month. Early in this month she discovered how to snap the rubber string attached to one of her balls. She held the ball down with one hand, and snapped the string with the other; an operation in which a right-handed adult would be sure to make

a distinction in the use of the hands. The child made none. It seemed a matter of indifference whether she held the ball with the right hand and snapped the string with the left, or vice-versa. Of course an accurate count might have revealed a difference which was not apparent to inspection.

A little after the middle of the eighth month, the child began waving 'Bye-Bye.' She learned to do it in connection with starting for a ride, an experience which gave her great joy. In taking her out to her cab, the nurse always carried her on the left arm, leaving the child's left hand free, and as a result, she learned to wave 'Bye-Bye' with the left hand. A few days later we noticed that if her left hand was held, she refused to wave, but did so joyfully as soon as the hand was released. Early in the ninth month, she would wave 'Bye-Bye' with the right hand if the left were held, but still used the left hand if it were free. My subsequent notes on the point are meager, I regret to say, but I know the general course of events. Somewhat later she passed through a stage in which she used either the left hand or both hands in waving, but never the right hand alone. Then gradually she began using the right hand occasionally, and by fifteen months, she had ceased using the left hand, and waved habitually with the right.

But long before her right-handedness had conquered habit in the matter of waving 'Bye-Bye,' it was perfectly evident in other activities. Toward the close of the eighth month, she began playing ball. She had a ball so large that it was a continual matter of surprise that she could make it stick in one hand. Sometimes she picked it up with both hands, but during the first days she always threw it with the right hand alone. After about a week of ball playing, she began using the left hand, or both hands occasionally in throwing, but the right hand has retained its ascendancy without a break.

During the ninth month, right-handedness began to be apparent in her ordinary activities, and by the end of the month I have the note 'evidently right-handed even to casual observation.' When in her seventeenth and eighteenth months she began using spoons and pencils, she used the right hand almost as exclusively as an adult, and was awkward when she tried

the left hand. No attempt was made at any time to teach the use of the right hand. The only factor in her surroundings of which I was aware, that might have influenced the process — that of the arm on which she was habitually carried — favored the left hand, as I have pointed out. But even the impetus given to the use of the left hand by this factor, as shown in waving ‘Bye-Bye,’ was soon overcome by her native right-handedness.

In recording which of the discs or forms was taken, it soon became apparent that regardless of the hand used, the right-hand position possessed an independent attraction for the child. In the selection of the colors, where the right hand was used 206 times and the left 194, the right-hand *position* was selected 285 times, and the left 183. The difference is even more marked in the case of the square and circle, where the material itself was indifferent. Out of 70 choices, 56 were for the right-hand position and only 14 for the left. The fact that the right hand was the one used for reaching, and that in this case some of the series were presented at reaching distance, tends to increase the choice of the right-hand position in this case, but even so, the right-hand position has an independent attraction, for it is chosen more times than the right hand is used. The right-hand position was taken 56 times, while the right hand was used but 40 out of the 70 times.

I must confess myself at a loss for an explanation for this preference for the right-hand position. It could not have had to do with the child’s posture, or with her relation to the light, for those factors were varied from day to day. The tests were not all made in the same room, and the child sat sometimes in a high-chair, sometimes on a couch, and sometimes on the floor. The only requirement was that she should be in an easy position, with both arms free to move. The only suggestion which seemed at all probable was that it might be conditioned by her eyes, though as far as I could judge, or could test them, her eyes seemed not only normal, but unusually good. They were very well coördinated at an early age, and gave more evidence of distant seeing than is usual at so young an age.

So far as their application to theory is concerned, the tests are one more proof of the already accepted view that right-

handedness must be a normal part of physiological development, not a phenomenon explicable by training. The preference for the right-hand position, in excess of that for the use of the right hand, suggests a query as to whether the eyes could play a rôle in the development of right-handedness, but the query is scarcely worth making so long as the observation is an isolated one. The theory that the speech center and the neighboring one for the right hand in the left hemisphere are associated in development, received some support in this case. The period when the tests first show a preponderating use of the right hand in picking up the colors (the middle of the seventh month), is just the one when the child began to babble syllables. Before that, the variety of sounds she made had been small. But the early development of a decided right-handedness (ten months) has not been accompanied by an early acquisition of speech. Though she said her first word at about ten months, she has acquired them very slowly, and has but fifteen or twenty words, many of them very indistinctly pronounced, at eighteen months.

# STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF THE UNIVERSITY OF CHICAGO.

## THE AUTOKINETIC SENSATION.

PROFESSOR HARVEY A. CARR.

This paper presents some of the results of a series of observations designed as preliminary to a more extended and detailed investigation of the autokinetic illusion. This subject has been selected by a graduate student of this department for a doctorate thesis. As a consequence, a separate publication of these results was deemed advisable. Historical references and discussions of explanatory theories have been omitted as much as possible.

The autokinetic sensation refers to the illusory movement presented by a small light of weak intensity when fixated in a dark room. Our tests are designed especially to determine the conditions governing the direction, speed and uniformity of these movements. Many observations of previous investigators have been confirmed during the progress of the experiments, but these results will not be mentioned except where modifying statements need be made. Four subjects, *C*, *A*, *W* and *F*, were used at times. At least two subjects were used in every test.

### A. EXTENT AND VELOCITY.

Greater extent and velocity of movement were observed than had been formerly reported. Charpentier<sup>1</sup> states that the illusion may attain a length of 30° or more. Subject *C* reported illusions of at least 65° in extent. These maximum results were obtained under conditions of marked ocular fatigue and constrained eye position. Often the light would move upward to such an extent that it would appear almost vertically above the head. Under conditions of previous constrained eye position (see Sec. F.), the light moved with an apparent velocity of

<sup>1</sup> 'Sur une illusion visuelle,' *Comptes rendus*, CII.

15°–20° per second, giving the impression of a sky rocket or a rapidly moving shooting star.

#### B. UNIFORMITY OF OCCURRENCE.

One subject, *F*, out of the four did not experience the illusion under any condition. The fact that some persons cannot obtain the phenomenon has been reported but no idea has been given as to the percentage of such cases. The fact that the direction, rapidity, regularity and extent of the movements vary with individuals was confirmed. An individual treatment of most of the results is thus rendered necessary.

#### C. TYPES OF ILLUSION.

The illusion may occur under three typical conditions: (1) The point of fixation remains with the light and participates in the illusory movement. (2) The point of fixation apparently remains stationary and the light moves away from it in some direction. (3) A combination of the above two types occurs. Both the light and the fixation point participate in the illusion, but unequally. The fixation often lags behind the light, especially for constrained positions of the eyes in the socket.

The first type of movement is the only one specifically mentioned in the literature. The second type could not be obtained by subject *W*. Observers *A* and *C* can obtain any type of movement at will.

The second type of movement differs from the first in several respects. Its extent is less under similar conditions. The maximum amount of movement observed was 20°. The average extent was approximately 10° in our tests. The amount varies with individuals, however. Its rapidity is much slower than that of type I., being barely perceptible in many cases. Its direction is more uniform during any one observation, and more constant for successive tests for the same eye position. This statement is especially true for the more central eye positions. Its velocity tends to be jerky and discontinuous as contrasted with the relatively smooth, flowing and continuous motion of the first type. The illusory movement finally stops, the light remaining stationary at some secondary position. This phe-

nomenon never occurred in the first type for our subjects, though it has been reported in the literature of the subject.

#### D. APPROPRIATE EYE MOVEMENTS.

No appropriate eye movements occur in the first type of illusion. By an appropriate eye movement is meant one whose length approximately equals the extent of the illusion and whose speed and direction bear some constant relation to the velocity and direction respectively of the illusion. Slight irregular twitchings of the eye are of course present, as one might readily infer from the recent photographic work upon fixation and eye movement. The eyes are thus relatively motionless during the illusion and the apparent movement of the fixation point is an illusion.

Appropriate eye movements do occur in illusions of the second type. The eyes move to the same extent as the light but in the opposite direction. The fact that the fixation point appears to remain stationary is an illusory phenomenon.

The position of the point of fixation in relation to the light thus indicates the relation of the light to the eye. The light is foveally perceived when the fixation point and the light coincide in position, and it is peripherally perceived when they do not coincide. The apparent distance of the light from the fixation point indicates with some degree of accuracy the extent of the deviating eye movement. The apparent direction of the light from the fixation point indicates in a large majority of cases the general direction of the deviating eye movement. Subject *A* reported in one illusion of the second type that the light had moved upwards and to the right, *i. e.*, the fixation point appeared to be situated below and to the left of the light. As a matter of fact the eyes had moved straight to the left.

This question as to the presence or absence of appropriate eye movements as a conditioning factor of the illusion has been a subject for some diversity of opinion. The writer<sup>1</sup> assumed the existence of such eye movements on the basis of a striking similarity between this illusion and that due to eye closure. Charpentier<sup>2</sup> argues against the presence of eye movements.

<sup>1</sup> *PSYCH. REV.*, Mon. Sup., Vol. VII., p. 94.

<sup>2</sup> *Ibid.*



He found that he could control the direction of the illusion to some extent by attending to some peripheral position while still maintaining fixation of the light. The illusion is deflected in the direction of the peripheral attention. Charpentier contends that any eye movement under the conditions would be directed toward the position of attention, *i. e.*, would possess the same direction as the resulting deflection of the light. Conditioning eye movements are thus impossible because their direction should be opposite to the resulting deflection according to known principles of psychological optics.

After the light had moved some distance, Bourdon<sup>1</sup> rotated the eyes back so as to fixate the original fixation position and then suddenly illuminated the room. It was found that the eyes were directed away from the light to the extent of the voluntary rotation. Consequently, the light must have been foveally perceived before the rotation and no eye movement was present during the illusion.

Our conclusions are based upon three kinds of experiment, all of which gave uniform and consistent results. (1) Bourdon's method of turning on the illumination of the room during the illusion was used. This was often suddenly done by an assistant at a time when the observer was not expecting it. Invariably, the eye was found to be fixating the light in the illusion of the first type. In the second type, some secondary position was fixated. The relative position of the fixation point and the light during the illusion was generally found to be the same as that obtaining after the room was illumined. In one case previously mentioned, there was a marked discrepancy between the directional relation of the fixation and the light in the two conditions of observation. (2) The negative after-image method was used. The observed light was 1 cm. square. Around this was placed strips of colored paper 1 cm. wide, forming a square 3 cm. in dimensions. The light, surrounded by the colored strips, was fixated with full illumination of the room until a strong negative after-image of the colored strips had had time to develop. The illumination was now turned off, only the light remaining visible. During the course of the

<sup>1</sup> *La Perception visuelle de l'espace*, p. 333 ff.

illusion the negative after-image developed and disappeared. In illusions of the first type the after-image remains with the light and participates in the illusion. With the second type, the after-image appears motionless while the light exhibits movement. In every case, the after-image is located at the point of apparent fixation. In the first type there are slight relative movements of the light and after-image indicative of small irregular movements of the eye-ball whose significance will be discussed later. (3) The eye was also directly observed during the illusion by an assistant. The subject's head was held by a mouth-bit head-rest. Before one eye was placed a small wooden frame over which was stretched a series of horizontal and vertical threads 3 mm. apart. The assistant was seated to one side and in front of the subject, a second head-rest being used to aid in accurate observation. While the room was illumined, the subject fixated the light and the assistant sighted through the system of threads at some distinctive mark on the eye-ball, *e. g.*, the line of demarcation between the iris and the sclerotic. The assistant illumined the eye observed by means of an electric flashlight whose light had been diminished in intensity and narrowed in area. This spot of light illumined the eye-ball sufficiently for accurate observation, but its position and intensity were such as not to destroy the illusion. The room was now darkened, and the eye was observed during the course of the illusion for as long a time as desired. Two observers and two subjects were used. In illusions of the first type, no movements of the eye-ball other than irregular spasmodic twitchings were observed. In the second type corneal movements were always present, whose direction, speed and extent always bore some definite relation to the corresponding characteristics of the illusion. It is to be recalled that the illusions of this type are slow, uniform in direction, and that the illusion finally stops, the light remaining in some secondary position. The conditions are ideal for the corneal observations. Corneal movements of 3-4 mm. in extent and of various directions were observed.

## E. INFLUENCE OF EYE POSITION DURING THE ILLUSION.

The position of the eyes in the head during the illusion is a determining factor. The strength of this position factor and the kind of influence it exerts vary with individuals. It may modify the occurrence, the length, the direction and the velocity of the movement.

Its most marked influence was upon the direction of the illusion. Any position tended to cause the illusion to move in the same general direction as that of the illusory movements due to winking for the same eye position.<sup>1</sup> This was true in a general way for three subjects studied. A different result might be obtained with other subjects. The uniformity of the direction during any one movement is much greater for some positions than for others. The direction of the illusion for a series of tests in the same position is more uniform for some positions than for others.

One subject could obtain the illusion only for certain eye positions.

The extent of movement possible varied with eye position, but the influence of any position in this respect was an individual variant.

The speed of the movement seemed to vary slightly according to the position of the eye, the effect being more noticeable for some observers. The uniformity of the velocity during any one illusion and for a series of illusions with the same position varied markedly according to the positional factor.

The subject's head was held stationary by a mouth-bit head-rest. The wall of the room was used as a background. Two constant eye positions were determined, the primary position and the zero points.<sup>2</sup> The light was placed at various positions on the wall, whose direction and distance from the primary position and zero points were known. Tests were made for each position for a number of days in succession. The direction of the illusion was plotted on a piece of paper immediately after the test, and its various characteristics as to speed, uniformity and extent of movement were noted.

<sup>1</sup> Carr, 'Visual Illusion during Eye Closure,' *PSYCH. REV.*, Mon. Sup., Vol. VII.

<sup>2</sup> Positions where no illusion occurs during eye closure.

Observer *C* performed 132 tests on 11 fixation points for the first type of illusion. There was a marked tendency for the light to move toward the periphery of the field. The direction of the illusion is thus different for the various positions of the eye in the head. The peripheral component predominated in 88 per cent. of the tests. This peripheral tendency increased with the distance of the light from the center of the field. The average percentage for all positions within  $30^\circ$  from the center was 75. Outside of the  $30^\circ$  zone the average percentage was 95. For extreme fixation positions the percentage was 100.

For a series of tests for the same position taken on successive days, the direction may vary within certain limits. These limits of variable direction decrease in proportion to the distance of the fixation position from the center<sup>1</sup> of the field. Near the center the limits are as high as  $80^\circ$ . This amount decreases to  $43^\circ$  and  $15^\circ$  for positions situated  $15^\circ$  and  $30^\circ$  respectively from the center. Outside of the  $40^\circ$  zone the limit of variability is practically *nil*.

The rapidity and extent of the movements are as a general rule greater for extreme peripheral positions. The speed and direction of any movement are more constant and uniform for peripheral positions. Inside of the  $30^\circ$  zone the direction and speed of the movements are highly variable from time to time. The motion is jerky, erratic and aimless in character.

A peripheral direction obtained for the illusions of the second type, the eye movements being directed toward the center of the field. For peripheral positions the movements were longer, and their direction and velocity were more regular.

The results of subject *A* are similar to the above. A total of 175 tests upon 17 eye positions were taken. A peripheral tendency was present in the majority of cases. This tendency increased in proportion to the eccentricity of the fixation. The percentages were 55, 73 and 97 for the distances of 7, 15 and 30 degrees respectively from the zero points. The limits of variability for successive tests in the same position decreased

<sup>1</sup> The center of the field usually refers to the primary position. It was impossible to determine accurately the functional center, *i. e.*, that position whose directional influence upon the illusion is *nil*. It seemed to lie somewhere between the primary position and the zero points.

with the distance of the position from the center. Approximate constancy of direction obtained outside of the  $35^{\circ}$  zone. Inside of this zone, the rapidity was relatively slow, the extent small, and the velocity and direction highly irregular. The peripheral direction obtained for illusions of the second type. The length and regularity of speed and direction varied with the eccentricity of the fixation position.

Both of the above subjects possess zero points, and the directions of the illusory movements due to eye closure generally possess a marked peripheral component. This peripheral directive tendency also obtains for the autokinetic illusion, and the strength of the position factor increases with the eccentricity of the position for both kinds of illusion.

With subject *W*, the illusion moved upwards in every case except one. The position of the eye in the head modified the direction slightly. The movements were slightly inclined to the right for all positions in the right half of the field. A leftward tendency was noticeable for positions in the left half of the field. In the lower half of the field, no illusion could be obtained in 50 per cent. of the trials. In the upper half of the field the movements were much longer and faster than in the lower half. The directions of the autokinetic illusion are similar to those of the winking illusion for the subject. No illusion of the second type could be obtained.

#### F. EFFECT OF PREVIOUS EYE POSITION UPON THE ILLUSION.

An eye position not only exerts a characteristic influence upon the illusion while the eye is in that position, but it leaves an after-effect which modifies the direction and velocity of the illusion in a characteristic way for subsequent eye positions. The behavior of the light in any position thus may be a function both of that position and previous eye positions.

The *duration* of the after-effect varies between zero and one minute, and it is a function mainly of the distance of the position from the center of the field and the length of time the eye maintained the position. To a slight degree it is also a function of the direction of the position from the center.

The *directional* effect of a previous fixation position is a

function of the distance and direction of that position from the center of the field. The field of regard may be divided into zones, an inner and outer. Any position 'a' in the inner zone tends to make the illusion for subsequent positions move in the same direction as that of 'a' from the center. Any position 'b' in the outer zone tends to make the illusion for subsequent positions move in a direction opposite to that of 'b' from the center of the field. This opposite direction of movement is quite rapid and is always followed by a slow return movement. The directional effects are in no way dependent upon the duration of the original fixation.

The effects upon *velocity* are a function mainly of the distance of the position from the center and the duration of the fixation. To a slight degree it is a function of the direction of the position from the center.

The after-effects of a position are thus neither similar nor antagonistic to the effects of that position during the illusion (see previous section).

The above generalizations are based upon two series of tests upon subject C. They are not to be regarded as possessing any universal validity. The light was placed in the center of the field where the effect of the position factor is at a minimum. The general direction of the illusion at this position was determined in a series of five successive tests at the beginning of each day's experimentation. These directions furnish the norm for comparison. During full illumination of the room, the observer was asked to fixate some peripheral position for a definite period of time determined by a stop watch. At the end of this period, the assistant suddenly turns off the illumination. The subject is directed to rotate the eyes and fixate the light immediately upon the darkening of the room. A series of seven positions 30 cm. apart in four directions from the central light were chosen. The distances of these positions from the center will hereafter be expressed in angular terms in reference to eye as the spherical center. Each position was fixated for seven different periods of time, varying from a second up to 2 minutes. This makes a total of 196 tests. This whole series was repeated. The direction of the movement was plotted

immediately after the test. These directions were compared with the normal direction for the central position.

As to the after-effects of position, the field may be divided into two zones, an inner and an outer. The inner zone is elliptical, the shorter axis being the vertical. The lengths of the two axes are approximately  $45^\circ$  and  $60^\circ$ .

Previous fixation of a position in the inner zone deflects the light to some extent in the direction of that position, provided any influence is present. Fixation of a spot  $23^\circ$  to the right or left of the light results in a deflection of the illusion toward the right or left respectively. This is termed an illusion in the *same* direction. An effective fixation of a position in the outer zone produces at first a rapid movement directed away from that position. This is followed by a slow return movement in the same direction. Fixation of a position  $40^\circ$  above the center produces at first a rapid downward movement which is succeeded by an upward movement. This is termed an illusion of the *opposite* direction to distinguish it from the effects of positions in the inner zone. The influence of the outer zone is more pronounced and characteristic than that of the inner zone. It completely determines the direction of the illusion. This direction is constant for successive trials in the same position. Positions in the inner zone simply modify the illusion in a general way. There is no constancy for successive trials, a pronounced variable factor being present. Other effective influences are present besides that due to previous position. Thus the position leaves an after-effect which modifies the direction and velocity of the illusion for a central position.

The distance of 30 cm. between the positions was chosen arbitrarily. This gradation is too large to admit of any accurate delimitation of the zones. In the horizontal the directive results were the same at the  $29^\circ 45'$  position and the opposite at the  $40^\circ 30'$  position. At the  $35^\circ 30'$  position both tendencies were present, one or the other being predominant at times, or the two forces neutralizing each other. For this reason the  $35^\circ 30'$  position has been chosen as the horizontal limit. In the vertical the directive results were always the same for the  $23^\circ$  position and opposite for  $30^\circ$  position. The zonal limits are

evidently somewhere between these two values. The same results were obtained in both series of tests.

The occurrence, velocity and duration of the after-effect depend upon the length of time the position was fixated. The duration of the fixation exerts no directive influence, however. These results for horizontal fixation points are represented in Fig. 1. The ordinates represent the duration of the fixation in sec., and the abscissæ represent the distance of the position from the center in degrees. The dotted vertical line through the  $35^{\circ} 30'$  position represents the boundary between the two zones. (1) There is a minimum duration of fixation necessary to secure any result, and this period decreases with the eccentricity of the fixation position.

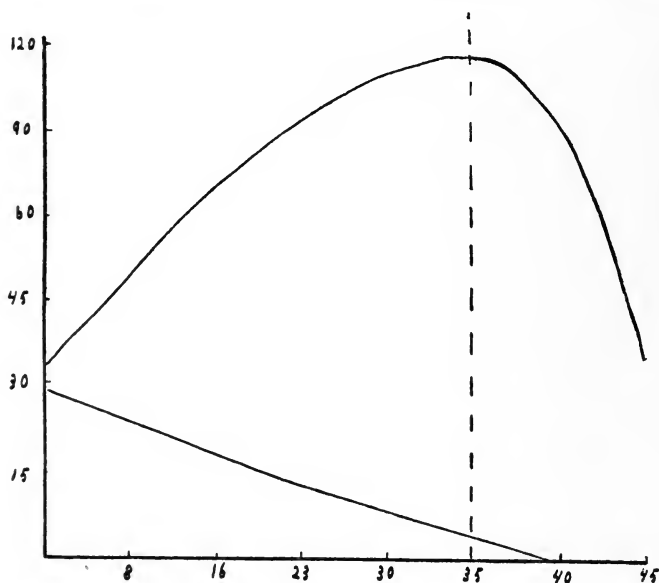


FIG. 1.

This is represented by the lower curve. Below this line, no after-effects can be secured. The duration intervals were chosen arbitrarily, and they are too large to admit of the construction of an accurate curve. The curve is based upon the facts that the period of effectiveness is less than 30 sec. for positions inside of the  $16^{\circ}$  zone, less than 15 sec. for the 23, 30 and  $35\frac{1}{2}$  degree positions, while a momentary fixation is effective outside of



the  $40^{\circ}$  zone. (2) There is a certain period of fixation necessary to secure the maximum of result for any position. This period varies with the position. The upper line of the figure represents this curve of maximum efficiency. The highest point of the curve — *i. e.*, the greatest time necessary to secure the maximum of result — is near the boundary line between the two zones. (3) Between the lower and upper limina of effectiveness, *i. e.*, between the two curves, the effect of any position increases in proportion to the duration of fixation. (4) After the maximum of effect has been obtained, further fixation decreases the effect. The effect for any position decreases in proportion to the height of the time ordinate above the maximum efficiency curve.

The preliminary character of the above tests is readily apparent. Merely the general character of the results has been stated. Many details and variations have been omitted. There are a number of facts which point to some such final statement as the following: Fixation of any position leaves two antagonistic effects, one tending to make the light move in the same direction and one in the opposite direction. The opposite tendency is the stronger but its duration is less than the second tendency. The duration and strength of each increases with the eccentricity of the position though not in the same ratio. The effectiveness of each for any position varies with the duration of the fixation, but not to the same degree. Such a conception is merely suggested as being the possible outcome of further experimentation.

The disturbance resulting from positions in the outer zone is very characteristic and noteworthy. The first movement in the opposite direction varies in duration from 1 to 21 seconds. It is very rapid; at the maximum speed obtained the light gave the impression of a rocket or a shooting star. Its initial velocity was often judged to be as high as 15–20 degrees per second. The subject was directed to fixate the light immediately upon the darkening of the room. The light had often moved 25–35 degrees before the eye succeeded in fixating it. This initial rapidity gradually decreases until the opposite movement stops and the return movement occurs. In this rapid phase of the

illusion there is a marked discrepancy between the apparent rate and duration of the movement and the amount of space traversed. Although the velocity may average  $10^\circ$  per second for at least 15 seconds, yet the light may not appear to move more than 210 cm. — an angular distance of  $45^\circ$ . The light moves in a series of spurts, *i. e.*, the velocity seems checked momentarily from time to time. At these moments one's idea of distance seems to undergo a transformation. During the first spurt the light moves rapidly through a certain distance, *e. g.*,  $40^\circ$ . During this period the extent and rate of movement are proportionate. The velocity now is noticeably checked for a moment, and at this time the transformation of conceptual space values occurs, for the light suddenly seems but  $10^\circ$  instead of  $40^\circ$  distant from its starting place. Another spurt occurs and the light now moves to a distance  $45^\circ$  from the center. This is followed by a second check in which the  $45^\circ$  instantaneously shrinks to something like  $15^\circ$ . This process is continued, though it is more marked in the initial phases of the movement. The above values are intended to be merely illustrative. As a consequence the light gives the impression of moving very rapidly without getting anywhere.

This opposite movement due to the extreme peripheral fixation can be obtained by subject *C* in ordinary perceptual conditions. The only difference is one of degree. The experience in daylight is very similar to that obtained by rotation dizziness.

In all the above tests the light has been located at the center of the field. The after-effects of a peripheral fixation will be the same no matter where the light is located. If '*c*' refers to the center of the field, '*a*' to some fixated position, and '*b*' to a peripheral location of the light, the general statement may be made that the after-effects of fixating '*a*' depend upon the distance and direction of '*a*' from '*c*,' and in no wise upon the spatial relation of '*a*' to '*b*.' This statement is based upon the results of numerous tests. The following are given as illustrations: (1) The light was placed  $15^\circ$  to the right of the center, and the eye fixated a spot  $30^\circ$  to the left of the center. A slow movement to the left resulted. (2) The light was placed at the center and a fixation point  $45^\circ$  to the left was chosen.

A sky rocket movement to the right resulted. The distance and direction of 'a' to 'b' were identical in the two cases, but the results were different in both direction and velocity. In the first case practically the same effects would have been obtained if the light had been located at the center of the field.

If the light is placed in some extreme peripheral position, where the illusory result is determined by the eye position during the fixation, two tendencies are apparent, that due to the position itself (see *E*) and that due to the effects of the previous position. These tendencies may enhance or neutralize each other. Consequently the illusory movement of the light at a position 'b' is a function of the distance and direction of both 'b' and 'a' from the center.

The after-effects of eye position are thus a disturbing factor which must be taken into account in determining the effect of the position factor (see *E*). A small interval of time (not more than 1 min.) should be allowed between successive tests. In the interval, the eye should occupy the central position. These precautions were not systematically observed in the tests described. Consequently, it is possible that some of the variability of results in the earlier phases of the movements may be explained in this manner. Such a factor could have no influence upon the *final* direction of movement because the course of the illusion was generally observed during more than a minute's fixation.

All of the preceding results refer only to subject *C*. The observations of *A* differ in a few details. Three zones of influence were evidenced. Positions in the inner and outer zones deflected the illusion in the opposite direction. A like direction obtained for the middle zone. The boundary between the two outer zones was elliptical, the vertical axis being the longer. The limit of the inner zone was between  $8^{\circ}$ – $16^{\circ}$  from the center of the field.

Subject *F* could not experience the illusion under these conditions of induced fatigue. But few tests were taken upon subject *W*. Previous eye position was effective to some degree but its influence was not marked. The few tests made do not allow of any general statements as to the directive character of the disturbance.

Evidently the after-effects of eye position vary markedly with individuals as do the effects during fixation. The character of the after-effects apparently is related in some way to the influence exerted during fixation, because observers *C* and *A* give similar results in both experiments. The relation is not one of directive antagonism, however.

These after-effects are probably to be considered as due to ocular fatigue. Constant fixation of a peripheral position can be maintained by subjects *A* and *C* only with strain and effort. This necessary effort increases with the eccentricity of the position. Extreme positions soon induce fatigue almost to the point of exhaustion. After inducing fatigue, a rotation of the head around the anterior-posterior axis changes the direction of the disturbance. If the normal results of a fixation is a movement to the left, a 90° rotation of the head on the left shoulder will cause the light to move downward. This evidences the fact that the effects of a fixation are resident in the ocular muscles.

#### G. SOME FACTORS PRODUCING VARIABILITY OF DIRECTION.

In the two preceding experiments to determine the influence of eye position, an irregularity in speed and direction during the illusion, and a variability in direction for successive tests on the same position were noted. This variability was extremely pronounced for subjects *C* and *A* for central positions where the influence of eye position was minimal. This variability indicates the functional efficiency of other determining factors. The remaining tests were designed to determine the nature of such variable disturbing factors.

1. *Influence of Retinal Rivalry*.—The average direction of movement for monocular and binocular conditions of observation was determined for a number of positions from a series of successive tests. The average directions for the three conditions of observation differ slightly. Right or left monocular vision with subject *C* tends to deflect the direction to the right and left respectively. The binocular direction seems to be either a mean between the other two, or else to be coincident with one of them. One is not justified in making any general statement other than that some directive influence probably exists.

In binocular vision the two eyes are known to alternate frequently in visual effectiveness, *i. e.*, to approximate monocular conditions. If the direction varies for the three conditions of observation, it is probable that these frequent alternations of ocular effectiveness should result in some slight disturbances in the direction or velocity of the movements.

2. *Effect of Voluntary Eye Movements during the Illusion.*

— The subject was directed to rotate the eyes from one edge of the light to the other from time to time during the illusion and to note any coincident disturbances in direction or velocity. The eyes were rotated in both the vertical and horizontal planes. The tests were performed for various eye positions. Two lights were used, a square light of 1 cm. dimension, and a light in the shape of a cross allowing movements  $3^{\circ}$  in length. The effects were similar for both lights. Frequently no disturbance was noted. A slight momentary influence of the same direction as that of the eye movement was generally present. The disturbance was momentary and never more than one half degree in extent. No permanent influence upon the direction or velocity of the illusion was ever noted. These eye movements may thus result in a slight temporary irregularity of direction or velocity.

Observations were made while attempting to fixate a distinct point in the light. These results were compared with those obtained when the fixation was allowed to wander over the light at will. No differences in direction or velocity could be noted between the two conditions of observation.

The eye was rotated toward the light from various directions at the beginning of the illusion. Care must be taken to choose such distances that no after-effect of the previous position is evident. The effect of the eye movement *per se* is desired. A slight temporary disturbance of the same direction as that of the eye movement was generally observed. No permanent effect was exerted.

The fixation was suddenly shifted for various distances and directions away from the light during the illusion. Such movements of more than  $2^{\circ}$  tend to destroy the illusion temporarily, the light appearing to be located nearer to its original position.

If the eye is held stationary in the secondary position the illusory motion again appears. The illusion is now slow and small in extent and its direction is similar to that obtained when the light is located at the secondary position. The illusion is similar to that of the second type. Subject *W* reported that during the voluntary shift the light appeared back in its original position without any sense of motion being aroused. With subject *C* the light generally returned only part way toward its initial position. In approximately half of the tests the return was accompanied by an exceedingly vague and fleeting sense of motion, a phenomenon of which one could make no very confident statement. Large and rapid voluntary sweeps of the eye away from the light during the illusion thus awaken a more accurate impression of the position of the eye in its spatial reference. They transform one's conceptual system of space. This sudden transformation of conceptual space relations often awakens an impression of a sudden fleeting motion on the part of the light. This transformation of conceptual spatial values is identical with the phenomenon observed during the rapid movements due to previous eye position.

3. *Effect of an Objective Movement of the Light.* — During the illusion the light was oscillated in the horizontal direction. The test was made for various eye positions. The extent of movement was either  $30'$  or  $1^{\circ} 20'$ . The rapidity of the oscillations was varied. For those positions where the illusion normally moves rather rapidly in a vertical direction, one obtains a spiral or cork-screw effect, a composite of the horizontal oscillations and the vertical movement. For positions where the light moves rapidly in the horizontal, the oscillations alternately accelerate and retard its velocity. When the illusion is normally very slow and the oscillations are quite rapid, one can detect the presence of the illusory factor, but its continuity and regularity is destroyed or rather overshadowed by the rapid oscillations. So far as can be observed, objective oscillations do not disturb the illusion in any way, for the total result is a composite of the oscillations and the normal illusory motions for that position. If any effect is exerted, it tends to decrease the velocity.

#### 4. *Effect of Involuntary Eye Movements during Fixation.*

— It is axiomatic that absolute constancy of fixation is impossible. Slight irregular involuntary shiftings of the eyes are almost continuously present. Slight eye twitchings were directly observed in these tests. Some writers are inclined to attempt a complete explanation of the autokinetic phenomenon in terms of these involuntary eye movements. The relation between these involuntary eye movements and the illusion was studied by the negative after-image method. The method was described in Sec. D. In illusions of the first type a previously induced negative after-image remains with the light and participates in the illusion. However, there are slight irregular relative movements or oscillations due to the involuntary shifts of the eye-ball. It is not contended that these oscillations give information of every eye movement, for some of these latter may be so small that the consequent shift of the light stimulus over the retina is below the spatial limen, *i. e.*, non-effective upon the location of the corresponding perceptual object. The oscillations do represent the *spatially effective* movements. The distance between the edges of the light and the concentric after-image was 1 cm. A rather accurate estimation of the length and direction of the oscillations was thus possible. A simultaneous comparison of the presence, extent and direction of the effective eye movements and the length, direction and velocity of the illusion is thus possible. A series of observations was made by subjects *C* and *A*. Various eye positions were used, and hence all typical characteristics of the illusion were studied. Oscillations occurred at some time in every test, though there were periods in which none could be detected.

Generally no effective eye movements occur during the marked and significant directional changes of the illusion. When such eye movements are present, their direction bears no constant relation to the illusory phenomenon. Fifteen observations were made during a marked and permanent change of direction. In most of the cases the direction was completely reversed. No oscillations were detected during or just previous to thirteen of these directional changes. The movement was reversed from an upward direction in the two remaining obser-

vations wherein oscillations were present. The oscillations were in the horizontal in one case, while the fixation shifted upward 5 mm. in the other.

No difference could be detected in the behavior of the illusion for the periods wherein no oscillations were present as compared with those periods in which they were detected.

The direction of the oscillations bore no constant relation to that of the illusion. As to direction the oscillations are very irregular while the illusion may be constant and uniform in this respect. Periods sometimes occur in which the illusion is very irregular in both velocity and direction while the oscillations are normal in character.

The length and rapidity of the oscillations vary markedly from time to time. The average amplitude was approximately 5 mm. around the center of fixation. No movement greater than 1 cm. (16') was observed. No correlation could be made between the extent and rapidity of the oscillations and the velocity of the illusion. The oscillations were possibly less pronounced for peripheral than for central fixation positions. The speed, length and uniformity of the illusory movements are the greater in the former case. Observations were made during the 'shooting star' effects previously described. The oscillations were slow and small during these rapid velocities.

Oftentimes no oscillations occur, yet the light and the after-image are not concentric. In other words the light is perceived paracentrally. The extent and direction of these deviations from foveal fixation were noted in relation to the various characteristics of the illusion. No correlation between the two could be made.

On the whole, no correlation was found between the various characteristics of the illusory movements and any features of the oscillations. On the contrary, the results suggest that possibly steadiness of fixation may favor the velocity and uniformity of the movements.

In this connection it may be well to note that some observers cannot obtain the illusion under any conditions. Others find it impossible to experience the illusion for certain eye positions. One would hardly dare venture the opinion that involuntary eye



movements do not occur in these cases, that some people can maintain absolute fixation. In view of these facts, the hypothesis that regards the eye movements as the sole and sufficient explanation of every phase of the illusion is impossible. Otherwise why should the bulbular twitchings beget an illusion in one case and not in another?

One feature of the oscillations merits additional comment. Since the negative after-image corresponds to a purely retinal process, it would seem that any relative movement of the after-image and the light due to eye twitchings would be exhibited by the after-image. As a matter of fact this was not true. The oscillations were due to irregular movements on the part of the light. This fact might seem at first thought to disprove the assumption that the oscillations represent eye movements. This possible conclusion is not justified for two reasons: (a) No oscillations were ever observed between two concentric negative after-images. (b) When two lights move relatively to each other, the movement for some reason is ascribed to the smaller object. This general statement is the result of three series of observations. The first refers to the observations already reported wherein a small light was perceived in the center of a large after-image. The light exhibited the motion. In the second series, a small negative after-image was developed in the center of a large light. In this case the relative motion was invariably exhibited by the after-image. In the third series two objective lights of equal intensity but of unequal size were perceived in an otherwise darkened room. They were situated some 10 cm. ( $2^{\circ} 45'$ ) apart. The assistant could move either light a distance of 4 cm. The observer was allowed to fixate the lights as desired and he was requested to judge as to which light exhibited movement. Any motion on the part of the small object was correctly perceived. With a medium velocity, any movement of the large light was interpreted as belonging wholly or in part to the small object. Such complete transference of the motion occurred in 80 per cent. of the tests. If the motion of the large object be small in extent and slow and uniform in velocity, the illusory transference will persist in spite of knowledge on the part of the observer.

5. *Effect of Eye Closure upon the Illusion.* — Involuntary blinking occurs quite frequently during the course of the illusion, and from casual observation one suspects that these lid movements are influential in determining the irregularities of speed and direction which so often characterize the illusory experience. Three series of observations were made to test this point. (a) Blinking was inhibited for as long a period as possible. (b) Binocular and monocular winking was initiated voluntarily. (c) Prolonged and intensive eye closures were instituted. All eye positions were studied in this manner and these periods were compared with normal conditions of observation. Inhibition of the involuntary blinkings exerted no apparent influence upon any characteristic of the illusion. Voluntary winks were sometimes effective and sometimes not. The effect was more frequent for central eye positions. For peripheral positions the velocity of the illusion was occasionally checked temporarily but no directive influence was noted. A momentary disturbance of direction was observed in 50 per cent. of the tests for central positions. The direction of the disturbance varied with the eye position but it was constant for any position. The disturbance was slight and temporary, consisting of a sudden jump of about  $1^\circ$  on the average. A *permanent* change of direction coincided with a wink in but one case out of 50 trials. Prolonged and intensive closure gave similar results with the exception that the extent of the disturbance was greater. The effect marked merely the beginning of each successive innervation and it varied in amount with the strength of the innervation. An increase of the effect was obtained as long as the innervation could be increased in intensity.

One observation of *A* is noteworthy. The light had moved several feet. After winking, the light appeared to be only a few inches distant from its starting point. This is another illustration of that sudden transformation or rather shrinking of space values which we have previously noted.

6. *Voluntary Control of the Illusion.* — Charpentier noted that the direction of the illusion could be modified voluntarily while still maintaining fixation of the light. He accomplished this result by thinking of or peripherally attending to some

object located in the desired direction, or by thinking of performing a movement in this direction. All observers were able to produce the effect at times. No observer could secure the result in every trial. A sudden and unexpected sound often deflected the light in its direction. Bourdon reports the same results with several subjects. The effect was secured with difficulty in some cases. A preliminary movement of the eyes in the desired direction was serviceable in securing control. The effect often persisted only during the voluntary effort.

The possibility of voluntary control was confirmed by our tests. The control may be exerted upon either the direction, velocity or extent of the movements. Subject *W* was unable to secure any effect. Apparently the degree of control possible varies with individuals. Voluntary influences are more effective for central eye positions. The control is never complete or absolute. Certain given determining conditions are always present in comparison to which volition is relatively powerless. Volition is rarely ever immediately effective. Its influence is manifested slowly and the effect gradually increases during a prolonged effort. The disturbance of direction is temporary if the voluntary effort is exerted but for a short period. After the movement once gets well started along the desired direction, the effect may be relatively permanent, *i. e.*, the deflected direction may be maintained after the effort has ceased. The difficulty involved in voluntary control seems to consist in getting the desired movement started with a sufficient momentum. The light gives the impression of possessing an inertia difficult to overcome. Once the light gets thoroughly started, its own momentum carries it forward.

From the standpoint of explanation, the crucial point is not the *fact* but the *mechanism* of voluntary control, a question which has not been considered in previous treatments of the autokinetic phenomenon. Our introspective observations indicated that innervations or strain sensations of certain ocular and facial muscles are necessary and essential to volitional control; that mere volition, *i. e.*, the idea of the proper direction and the mere fiat or intellectual decision, is functionless; that the effect occurs only when the volition is translated into appropriate motor terms and

that the degree of effect secured is proportionate to the intensity and direction of these motor tensions.

In voluntarily deflecting the movement to the right, there was a rightward ocular strain which often became painfully fatiguing. The trunk of the body became set and rigid, the breath was held, the jaws were firmly set and the facial muscles were twisted and pulled over in the desired direction. With a downward deflection there was an appropriate pull of the ocular muscles, and the facial muscles around the cheeks, eyelids and brows participated in the downward movement. The secondary contractures of the muscles of the jaws and the trunk of the body were also present. The eyes did not move because they were innervated antagonistically. Yet the antagonistic innervations did not seem equal, the surplus of one, as it were, overflowing into the surrounding muscles. These facial contractions were directly observed.

The necessity of these tensions was indicated by the fact that they were invariably present, that the degree of influence exerted was proportionate to the intensity and extent of the contractures, and that the attempt to control the illusion without concomitant strain sensations was unsuccessful.

A series of tests was made to determine the functional efficiency of motor tensions upon the illusion. While observing the illusion in a normal manner, various groups of muscles were strongly innervated without any thought of or attempt at volitional control over the illusion. All resulting disturbances were noted.

(a) The light was placed in a central position for which the normal behavior of the illusion was known. While observing the illusion the subject was asked to listen passively to some continuous sound located in a peripheral position of the field. Passive attention means a mere awareness of the nature and position of the sound without the presence of any ocular or auricular tension. No influence on the illusion was noted.

The subject was requested to give active auditory attention to the sound. This involved auricular attention with no ocular strain. A prolonged and intensive effort of such a character generally deflected the illusion to a slight degree in the direc-

tion of the sound. The result was not pronounced nor did it occur immediately.

Visual and auditory attention was initiated. The subject was requested to listen intently and to visualize the position of the sound. As a rule a deflection of the illusion in the direction of the sound occurred. An appropriate eye-strain was generally detected in the effective tests.

(*b*) All of the muscles of one hand and arm were vigorously contracted. The attempt was made to confine the innervation to these members exclusively. Slight disturbances were noted in the majority of the tests, the extent and direction of which were irregular.

Deep and slow breathing was instigated, the breath being held as long as possible. Unequivocal results were obtained in but one test. Inspiration deflected the movement upward, a return movement accompanying the expiration. This oscillation occurred for every act of respiration in this particular test. Subject *A* could obtain no results.

A strong contracture of the jaws produced a noticeable downward deflection of the movement under all conditions. A release of the tension was followed by an upward movement. The effect was quite pronounced and it followed the innervation almost immediately. No influence was obtained by subject *A*.

A binocular strain of the eye as represented by 'staring hard' at the light gave a very slight downward effect in some cases. A similar monocular innervation produced an effect in the majority of cases. For the left eye, the deflection was down and to the left. The deflection for the right eye was down and to the right. These statements refer to subject *C*. Different directional results were obtained by *A*.

A unilateral strain of the eye in any direction resulted in a pronounced deflection in that direction. This refers to a pull or strain on the eye in a lateral direction without any consequent deviation of fixation. A pronounced strain of this character involves facial contractures in the same direction. The disturbance was proportionate to the intensity and duration of the innervation and the number of facial muscles involved.

The direction of the illusion for central positions may be permanently reversed by a continuous and intensive effort of twenty seconds duration. In fact, the illusion may be almost completely controlled for central positions. Marked alterations of direction and velocity can be effected for peripheral positions.

Fixation of extreme peripheral positions can be maintained by subject *C* only by strenuous effort which is painfully fatiguing. By alternately relaxing and increasing the innervating effort without deviation of fixation, the light can be made to vibrate to and fro through a distance of  $7^{\circ}$ – $8^{\circ}$ . The velocity of the movement can be controlled by regulating the degree of innervation. In this case the eyes are not strained away from the light, as described in the previous paragraph. The effort is directed rather toward a maintenance of the fixation.

Fatigue resulting from a prolonged ocular tension produces marked disturbances whose direction is opposite to that of the strain. While the room was illuminated, the observer fixated the light and strained the eye in some direction. These tensions were maintained for periods of thirty to sixty seconds. The subject was directed to relax the tension at the moment the room was darkened, but still to maintain the fixation. The illusion was observed and compared with the norm for that eye position. The illusion was deflected in a direction opposite to that of the strain in 75 per cent. of the tests. The duration of the disturbance varied from 30 sec. to 2 min. The effect was more pronounced for strains to the right or below.

A number of observations were made during a condition of a constrained position of the head in relation to the body. The body was twisted so that the line of vision was directed over one shoulder. The head and the eye maintained their normal positions in reference to the light and relative to each other because of the head rest. The illusion under these conditions was compared with the norm for a number of positions. Subject *C* reported a possible disturbance for some positions. The results were too slight to admit of any confident assertions. Observer *A*, however, reported an average deflection in direction of  $45^{\circ}$  in 90 per cent. of the tests. The direction of the disturbance was always opposite to that of the body torsion.

Charpentier has reported that with monocular observation, finger pressure upon the closed eye produced deflections whose direction was dependent upon the character of the pressure but independent of the position of the eye in the socket. Our tests did not confirm these results in *toto*. The direction of the disturbance was dependent upon both the direction of the pressure and the eye position. The amount of deflection varied from 30' to 5°. The influence was temporary and it occurred only at the beginning of the pressure. Inasmuch as the pressure is exerted upon the closed eye, it is difficult to conceive how the vision of the opposite eye can be effected unless through the medium of the binocular tension. Binocular adjustment is unitary and any force exerted upon one eye must necessarily effect the tension of the other eye. A readjustment of binocular innervation is needed to resist the interrupting force.

The effect of prolonged and intensive eye closure described in Section 5 is similar to the above phenomena and may be adduced in support of the theory of innervation. An influence was noted at the beginning of each successive innervation of the lid muscles of the closed eye. The effect varied with the strength of the innervation. That is, this effortful contracture of the closed eye influences the illusion as experienced by the opposite eye.

#### H. THEORETICAL SUMMARY.

One common theory attempts a *complete* explanation of the illusion in terms of the involuntary eye movements. These small movements which are highly irregular in direction and extent are conceived to be integrated in some fashion so as to mediate the sense of a large and continuous movement of the light. This theory may be conceived of in two ways: (1) That the effective principle is the consequent retinal shiftings of the stimulus which awaken the sense of motion and which are combined into a continuous whole. The eye movement theory is generally stated in such terms. (2) The effective factor may be regarded as the fluctuating motor tensions which lead to the eye-movements. This paper admits that such eye-movements may play some minor rôle in the illusion, but it denies that they may be regarded as the only or the most important factor in the phenomenon.

Theoretical considerations may be urged against the theory as the *sole* explanatory principle. Why should the eye movements become integrated? What is the mechanism of this unconscious integration? Why does not the integration occur under normal conditions of perception? How and why should the process of integration be modified by the factor of position, etc.? Why should the process of integration achieve such diverse results under similar conditions? Factual objections may be given. Some people cannot experience the illusions under any circumstances. Some people can obtain the phenomenon for some eye positions, but not for others. The illusion does not occur in normal perception. No one can deny the presence of involuntary eye movements under these conditions. If eye-movements produce an illusion in one case and not in the others, at least some secondary principle of explanation is necessary. The conception to our mind is simply preposterous.

The experimental facts on the oscillation of the light and negative after-image are adduced against the contention that these eye movements are the most important causal factor. It may be objected that the test is faulty inasmuch as only the spatially effective eye movements were correlated with the illusion, that many minimal movements escaped detection. The objection may be granted without necessarily invalidating the argument. (1) If the large eye movements, as our results indicate, are practically without function, it is illogical to ascribe any pronounced effectiveness to the minimal movements. (2) The contention is rather inconsistent in that it assumes that eye movements which are *spatially non-effective* as to oscillations are yet spatially effective in that they produce the illusion.

Other facts than those given in the section on oscillation may be cited. A series of observations in which a small point on the light was fixated was compared with one in which the eyes were allowed to wander at will over the surface of the light. The eye movements were probably more pronounced in the latter case. No difference in the behavior of the illusion in the two cases was detected. In all probability the eye movements were accentuated when the light was moved objectively, but the illusion was retarded. Retinal shiftings of the stimulus occur in



the voluntary rotations of the fixation over the light, in the winking test and when the light was oscillated. These retinal shifts were sensed as motion, but the movements were not incorporated as an integral part of the illusion. They stood out independent, as additions, as disturbances; they were not undiscriminated parts of a continuity. If a series of irregular shiftings due to involuntary bulbular twitchings can be integrated to form a one-directional unity, as the theory assumes, it would seem that these shifts due to winking, voluntary rotation, etc., should be incorporated more successfully into the integrated whole. The fact that they are not so incorporated leads one to question the integration theory. The results on the oscillations of the negative after-image indicated that, if anything, steadiness of fixation favors the velocity and uniformity of the illusory motion, that the retinal shiftings due to involuntary rotations are to be regarded as disturbing elements, and that integration does not occur.

In the way of a positive contribution to a theory of explanation, our experiments reveal four effective factors which are influential in determining the direction, velocity, and continuity of the illusory movements. (1) The position of the eye in its socket; (2) the after-effects of eye position; (3) motor strains as exemplified in the tests on voluntary control, and (4) the after-effects of such strains. The mutual relations of these four conditions are to be considered.

The second and fourth are similar in that the after-effects which are so influential upon the perceptual experience must be considered as resident in some way in the neuro-motor mechanism. Compare a normal illusion with that obtained for the same position after a previous constrained position or a unilateral strain on the eyes. Radically different illusory results are obtained in the two cases. The effects cannot be ascribed to a difference in the sensory conditions of the observations, for these are similar. The after-effects are independent of the position of the eyes in the socket. Turn the head on the shoulders and the direction of the illusory disturbance is changed. The after-effects must be resident in the neuro-muscular mechanism. It is immaterial whether these effects be termed fatigue

or whether they be located in the muscle or in the motor centers. The purport of the facts indicates that different neuro-muscular conditions of observation give diverse illusory results.

The first and third factors are similar in several respects: (1) they both involve effort and strain, (2) the light moves in the direction of the strain, and (3) the illusory effect varies in direct proportion to the degree of strain involved. For the third factor these statements are a summary of the section on voluntary control. Their application to the position factor needs additional comment. Observers *A* and *C* can maintain a peripheral fixation position only by a strenuous effort which, if prolonged, becomes painfully fatiguing. The eye tends continually to move back to a more central position. The effort is directed peripheralward and the resulting illusion is in this direction. The degree of effort involved varies directly with the eccentricity of the position, as does the illusory effect. In illusions of the second type, *i. e.*, those due to eye movements, the eyes are invariably forced back toward the center of the field. The contention that the eyeball encounters a resistance in assuming and maintaining any peripheral position, and that this resistance varies in amount and character with the position is supported by the results of a previous paper on the winking illusion. This illusion was found to be due to an enforced eye movement whose direction and extent varied with the eye position. The eye-ball is not a perfect sphere and its rotary and geometrical centers do not coincide. The surrounding fatty tissues thus offer a resistance which tends to force the bulb in a characteristic direction for any position of the eye in the socket.<sup>1</sup> Now eye position, for our three subjects, exerts the same influence upon both the autokinetic and the winking illusion, *i. e.*, the force exerted upon the bulb in any position by the surrounding tissues is proportionate and antagonistic to the extent and direction respectively of the autokinetic illusion. The muscular effort involved in overcoming this resistance in assuming and maintaining a position is thus directly related to the resultant illusory motion. The autokinetic phenomenon may thus be regarded as due to the elements of strain and effort involved. The effective prin-

<sup>1</sup> Carr, *ibid.*, pp. 75-77.

ciple may be either the neuro-muscular tension or the resulting sensory experience involved in maintaining the bulb in a position against the resistant forces of the fatty tissues.

If an altered condition of the neuro-muscular mechanism resulting from previous activity can modify the illusion, it would seem that the previous neuro-muscular tension itself should be highly effective. Now the illusory effects of tension are directly related to the after-effects of that tension. This fact leads to the assumption that the effective principle in strain and effort is rather the neuro-muscular tension than the resulting sensory component. This view would possess the advantage of unity and simplicity, in that it reduces all four effective factors previously enumerated to the one principle, *viz.*, that the autokinetic illusion is mainly determined by the changing neuro-muscular conditions involved in a continuous fixation. From the standpoint of this article, however, there is no objection to ascribing a functional efficiency to the sensory component involved.

The sense of motion is our awareness of a change within a system of space relationships. Motion is relative. The fixated light must move in *reference to something*. This does not mean that the subject must necessarily be overtly conscious of the basal term and its space relation to the moving object. It does mean that the basal term must be represented at least in the background of the total experience. Its existence is necessary to mediate the sense of motion.

In illusions of the first type, the point of fixation participates in the movement. The illusion refers to the object of foveal consciousness. The light cannot move in reference to any other visual object, for no visual points of reference exist. This possibility is excluded by the conditions of the experiment. The only possible basal term of reference is the space feeling of the body as represented by the kinesthetic and organic sensations and the tactual experiences due to contact with objects in the room. In these conditions of perception, the object of foveal activity stands in a constant space relation to the eye. This fact was demonstrated by the tests on eye movement. Consequently the eyes are the only possible means of connection between the visual object and bodily space. The eyes,

tactually and kinesthetically considered, are a part of the bodily space. A correct definition of the position of the visual object relative to the body thus depends upon a correct appreciation of the eye-body relation. Any unusual conditions of the eye-body relation must result in an erroneous sense of the position of the fixated light in reference to the body.

The point of fixation appears stationary with respect to bodily position in the second type of the illusion. As a matter of fact it has moved because the eyes have undergone rotation. The fundamental fact is not the apparent motion of the light but the *lack of motion* on the part of the fixation point. The illusion occurs on account of an erroneous sense as to the spatial relation of the point of fixation in reference to the body, and this is mediated through an erroneous appreciation of the eye-body relation. Both types of illusion are to be explained on a similar basis.

The above situation may be contrasted with that of normal perception. The point of fixation or the visual field in both cases stand in a given fixed relation to the bulb. In normal perception, the mutual definition of the visual and the organic space systems depends upon several coöperating factors: (1) Certain objects appear in both space systems. The brows, nose, hands, feet, etc., are both seen and felt. The body as felt is often in direct contact with objects seen. The hands are moved to touch and explore the visual objects. The two systems of space experiences thus overlap. Mutual adjustments and definitions are rendered not only potential but necessary. (2) Steady fixation rarely ever occurs. Through movements of the eyes, head or body, objects are continually entering into and disappearing from visual space. (3) Kinesthetic-tactual cues as to the eye-body relation are furnished by pronounced eye or head movements. Inasmuch as the visual space occupies a given relation to the eye, any appreciation of the eye-body space relation organically sensed aids the mutual definition of the bodily and visual space. By these three means one develops a rather accurate mutual definition of two space experiences. One's definitive reactions come to be relatively immediate, automatic and habitual. The attention is centered upon the results

and not upon the sensory means, or cues. The position of any foveal object is immediately defined in terms of right or left, above or below. (4) Each rotary eye adjustment, or ocular attitude, must develop in time from the above sensory cues a certain spatial meaning or significance, whereby it is possible, if necessary, to define the direction of fixated objects and to adapt the body thereto. These ocular attitudes form a fourth means for the mutual definition of the two systems of space. Inasmuch as our spatial definitions become immediate and automatic, it is probable that these motor attitudes play an increasing rôle in the ordinary adjustments to visual space.

In the autokinetic illusions, the first three of the above mediating connections are excluded by the conditions of the observation. Only the light is seen. Neither the body nor the objects in contact with the body are perceived. Constant fixation is maintained. The head and body are stationary. No new objects can enter the visual field. Eye or head movements sufficient to arouse adequate kinesthetic-tactual cues are eliminated. A continuous ocular attitude forms the only link between the two space worlds. All definitions of direction must be made on the basis of the spatial significance which these motor attitudes have already attained. Naturally the potential value and accuracy of such a mediating factor may vary with individuals.

The theory assumes that this remaining link, the motor attitude, may be modified by various influences during the course of a long fixation so that its directional significance is gradually altered. As a consequence the subject's definition of spatial relation undergoes gradual transformation and an illusion of motion results. A position which at first seems to be directly in front of the observer is finally defined as being to his right. Since it is the *meaning* of the ocular adjustment that changes, thus giving rise to the illusion, it is proper to say that it is one's *idea* of direction which is altered, or that it is one's visual *conceptual space world* which, in reality, moves. It does not follow from this proposition, that the directional position of the light is completely subject to voluntary control, varying at the slightest wish. Ideas possess their given fixed characteristics to varying

degrees. The idea of direction can be changed only by altering or modifying in some way the character of the underlying motor-adjustment, as was exemplified in the section on voluntary control. Complete or absolute control would only be possible when all four connecting factors were destroyed.

This general conception is supported by several observations already described. If any of the three connecting cues excluded by the imposed conditions are suddenly introduced, a quick transformation of directional values results. The light has apparently moved thirty degrees to the right. Illumine the room and this sense of 'rightness' is immediately transformed to the 'in front feeling.' The given relation of the light and the body has remained unaltered throughout. The changes have occurred in the directional significance of the light as expressed in verbal or bodily reactions. The change has been one of motor interpretation. Rotate the eyes, wink, or press upon the bulb sufficiently to awaken tactual cues as to the eye position, and one's idea of direction based upon a fatigued ocular attitude is modified and corrected to some degree. This process of modification, or of transformation of space values occurring during the illusion, also temporarily destroys the illusory motion, checks its velocity, or awakens a sense of a return movement. If a sudden change in one's idea of direction can destroy or check a movement, or even substitute in its place a sense of motion in the opposite direction, the assumption that the original illusory movement is due to a gradual change in the idea of direction is not illogical. These sudden revaluations of directional significance occurring from time to time during the illusion are but special cases of the gradual alterations which produce it.

Such a conception presents difficulties but it systematizes and unifies the facts rather successfully. Inasmuch as the eyes normally are in almost constant motion, their acquired spatial meaning is based upon a merely temporary fixation. During a prolonged fixation fatigue occurs and the attitude from the standpoint of volition keeps gradually changing. Objectively speaking, the adjustment does not vary, but this fact does not matter. It is the functional, the dynamic attitude that is important. The psycho-physiological mechanism of adjustment does vary. The

fatigue effects due to previous strain are likewise influential inasmuch as they demand compensatory adjustment. The strains and efforts cited under the section on voluntary control simply represent an abnormal adjustment. In illusions of the second type, the motor attitude is maintained though the eye-ball and consequently the fixation point do move. The point seems to maintain the same position throughout. The illusory interpretation of the position of the fixation leads to an erroneous interpretation of the position of the light. In such a theory involuntary eye movements can play but little part. Any movements large enough to awaken tactual cues would tend to destroy the illusion. The consequent retinal shiftings would be represented as disturbing fluctuations. If any aspect of these movements were integrated into the illusion, it would be the fluctuating motor tension. Outside of this, steadiness of fixation or lack of movement, will favor the velocity and continuity of the illusion.

#### ANNOUNCEMENT.

The first number of a new monthly, the *Journal of Educational Psychology*, is announced for January. The editors are W. C. Bagley, J. Carleton Bell (managing editor), C. E. Seashore, and G. M. Whipple (subscription \$1.50; Williams and Wilkins, Baltimore, publishers).



# THE PSYCHOLOGICAL REVIEW.

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## EVOLUTION AND CONSCIOUSNESS.<sup>1</sup>

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There is no problem of present-day science of more vital importance to the psychologist than the problem of determining the relation of consciousness to the general process of organic evolution. This problem touches the very existence of psychology. The physiologists and the biologists have long been contending that they can give an adequate scientific account of human life without using the term consciousness or any of its synonyms, and their contentions will become convincing unless satisfactory evidences are speedily adduced to show that consciousness is not a mere by-product of organic adaptation. Indeed, there are those who bear the name psychologist who are, in this matter, arrayed on the side of the physiologists and biologists. They tell us that nothing significant is added to the concept of adjustment or the concept of behavior by discussing psychical factors. All behavior is a simple sensory-motor process; those types of conduct which the unscientific man is wont to think of as intelligent and extra-organic are merely complex instincts or at most combinations of reflexes acquired under the stress of external excitations.

I am well aware that it is bold to set oneself in opposition to this tide of biological opinion. One is likely to get himself classified as an uncritical layman when he begins in this day and age to talk about consciousness as of positive importance in the evolutionary process. Or he gets himself suspected of adherence to sentimental dogmas such as moved the early

<sup>1</sup> Presidential Address before the American Psychological Association, December 30, 1909.

critics of Darwin and Spencer. I shall hope, however, to defend my position adequately against the charges of uncritical superficiality and sentimentality. I shall hope to show in strictly objective terms that consciousness is a product of evolution which continues in a higher form the movement which is manifest in all earlier adaptations. I shall hope to show further that as soon as consciousness was fully evolved the direction of all adaptation was radically modified. Finally, I shall defend the thesis that if any scientific explanation of human life is to be attained that explanation must be based on a thoroughgoing study of consciousness. The social sciences have sought in vain to base themselves on the general doctrine of organic evolution. The processes of human adaptation are different from those of animal adaptation just because human adaptation is determined in character by consciousness.

Let us turn at once to the considerations that justify the theses which I have formulated. The statement on which all students of evolution agree is that there has been a steady increase in the complexity of organisms. It has frequently been pointed out that the use of such terms as progress or improvement is not objectively permissible. The biologist will not say that the more complex organism is better or worse, that the path of evolution is upward or downward. The strength of the captor is good from the captor's point of view, but bad from the point of view of the captive. Complexity on the other hand is a purely objective fact and is not affected by the point of view.

What is the significance of this increase in complexity? The lesson is perfectly clear when we look at the concrete facts. By an increase in complexity the organism attains to an ever-increasing degree of self-sufficiency. Take the matter of temperature for example. The simple organism is utterly dependent for its body temperature upon the environment, while the complex organism can maintain within its complex body a standard temperature of its own. The advantage for range of life is obvious. The simple organism cannot move beyond narrow limits while the complex organism with its self-sufficiency in matters of temperature can safely move through wide variations of environmental temperature.

Another striking example of increasing self-sufficiency is furnished by studies of the reproductive processes. In the simple forms of life the offspring is exposed very early to the mercies of the environment. The parent organism has no adequate means of protecting the young. Gradually the parent grows more complex, and in the same degree better able to protect the offspring. There is an increase in the food supply deposited with the egg, and an increase in protective devices. The goal of this line of evolution is reached when the parent becomes sufficiently complex in structure to provide for the elaborate development of the offspring within the parent organism. The whole process of evolution is here seen to lead in the direction of self-sufficiency on the part of the organism. Instead of depending upon the chances of environmental conditions the organism builds up an environment of its own within which its reproductive processes may be brought to a high degree of completion before exposing the product to the external world.

Every organ of the complex animal bears witness to the truth that inner self-sufficiency is the end toward which organic evolution has been progressing. There are organs for the storing of energy so that the individual shall be relatively free from the necessity of securing immediate nutrition. There are organs for the secretions of chemical reagents which shall convert the raw material used as food into proper ingredients for the building up of body tissues. Organisms have always exhibited in their higher forms organs of mobility which make them free to move at their own initiative.

In all these cases the obvious significance of increasing complexity is increasing autonomy of the individual. The process of evolution has resulted in a more stable set of inner conditions which make it possible for the vital processes to go on without interruption or hazard from fluctuations in the outer world.

The formula which has usually been employed in describing the evolutionary process is the formula of adaptation to the environment. This formula has given great prominence to the environment and one gets the impression from reading much of the current writing that the term adaptation has been interpreted

to mean growing approach to the environment on the part of evolving organisms. It is not true, however, that individuals are growing more and more like the environment. On the contrary the higher the individual the less its organism resembles the environment. The fish is less like the water in which it lives than is the medusa. The fish is more complex, more independent of external conditions, more clearly differentiated from its surroundings. The principle of self-sufficiency is seen in such examples to be a much more exact description of the evolutionary process than is any principle which can be misinterpreted to mean conformity to the environment. It is strictly in keeping with the facts to say that animal evolution has been one continual progress away from the environment. The struggle for existence is a struggle to establish and maintain individuality. Let it be noted that this statement does not for a moment deny that organisms arose out of the environment or that they continue to draw from the environment their subsistence. What is stated is that organisms are drawn out of the environment, that is, they are in ever-increasing degree differentiated from the environment during their more and more complete evolution.

Such considerations as these prepare us to understand the importance of consciousness. Consciousness is a function which promotes self-sufficiency by literally taking up the environment into the individual and there remoulding the absorbed environment in conformity to individual needs. Consciousness is an inner world where the motives of individual self-sufficiency are dominant. When in this inner world the relations between elements of the environment are adjusted in such a way as to conform to private demands, then the individual is in a position to go forward in an aggressive way with an attack on the outer material world. This outer world may now itself be remodeled to conform to the inner pattern. The self-sufficiency of the conscious being thus becomes an accomplished fact through ultimate subjugation of the environment. Consciousness is no less a fact than the inner standard temperature of the body. In both cases evolution has prepared an inner set of conditions in which life is more advantageously promoted. In the case of consciousness, however, the evolutionary process has gone so

far as to produce a function which changes the whole balance of the world and puts the environment in a very real sense of the word under the control of the inner organized being.

These general characterizations of consciousness can be made clearer by following in brief outline the salient steps in the evolutionary process. The primitive function of irritability out of which consciousness was evolved consisted at first in a general internal commotion whenever external energy acted upon the organism. Heat, for example, heightened the inner molecular activity of the protoplasm, likewise light and the grosser forms of energy. Gradually these inner commotions became differentiated. The now differentiated type of inner commotion began to reflect in some measure the relation between the individual and the environment. If the individual did not gain in power of self-preservation through the particular form of inner commotion, there was a tendency for that particular type of inner commotion to cease. If, on the other hand, the inner commotion meant greater ease and certainty of self-preservation, it was preserved and elaborated.

While differentiation was going on, the various differentiated forms of irritability began to undergo an internal combination. When light stimulation set up one form of commotion and contact set up another, the two protoplasmic commotions underwent within the individual a combination. This combination was a unique event. It was a combination governed by the laws of protoplasmic behavior. Light sensation and contact sensations were combined within a single organism, not for the purposes of bringing together light and molar energy but for the purpose of promoting a new complex vital function, an inner function which should serve the ends of self-sufficiency. To describe such a combination as a mere adaptation of the individual to the environment is to fail in the apprehension of the real evolutionary significance of the change. The individual is becoming internally integrated and as an integrated individual presents to the environment a more and more highly individuated front. The individual is working out the problems of its life within itself. Even its relations to the elements of its environment are being worked out within itself.

The integration of inner processes of irritability become more complete when in addition to integrating commotions aroused by forms of external energy momentarily present, the inner organism began to contribute through its powers of retention commotions brought over from past stimulations and past inner combinations. These inner complications make strikingly clear the trend of evolution. The organism is not becoming like its surroundings, nor is it remaining dependent upon environmental initiative. The organism is not becoming subject to external laws of energy. It is rather evolving in a direction which leads it further and further away from the external world. Its inner processes are becoming more complex, more and more independent of external energy, more highly individuated. The result of all this evolution is that the individual comes ultimately to contain an inner world governed by laws of combination which are wholly different from the laws of the outer world.

Some students of science profess to be mystified by the assertion that the inner world produced by evolution is fundamentally different from the outer world in its character and in its laws. These persons should study their biology a little more closely. They will find that from the first, evolution has been pointing away from the physical world and its laws toward a self-sufficient individual governed by inner laws.

When the individual reaches a stage where inner combinations can be freely worked out without dependence upon the environment there is likely to appear an excessive exercise of this inner power. When a child realizes that in his inner conscious world relations can be easily readjusted he indulges in unlimited recombinations. The race too exhibits in its myths the same freedom in making up recombinations. In both the child and the race the exercise of powers of recombination for the mere pleasure of the exercise has a secondary effect. Consciousness is, by this exercise, more fully developed and while the utility of such early mental processes is not great, yet the stability of consciousness as an inner function increases and is ultimately assured through exercise. We shall never grasp the significance of some of the earlier periods of human evolution

until we recognize the long effort which was required to establish the function of mere recombination in the conscious life. When the method of recombination was fully mastered, then and then only was the race prepared to refine the function by a critical sifting of its products.

After the inner combinations which we have described came to some degree of perfection the last step of evolution was taken. The evolved organism began to strive for a reorganization of the environment in conformity to its own imaginings. In these imaginings the world had been put together in such a way as to make it appropriate to the individual's inner needs. If now the outer world could be made to conform to the demands of the inner world, if the environment could be remoulded on the pattern of inner desires, then the world would be a more favorable environment. The history of this effort to fit the outer world to an inner pattern is the history of human struggle with the environment. The savage pictured the ease of a land full of good things and free from enemies. At first he adopted the simplest expedient of realizing this dream. He thought of such a land as lying somewhere ready made and he wandered about ever hoping to find the world of ease. Finally, he came to see that he must make that world by improving what was at hand and by removing all that was inimical. Again, the savage pictured a being like himself behind the wind and rain and he adopted devices in dealing with wind and rain which would have conciliated himself. He was baffled beyond degree when his efforts of sacrifice and worship were of limited avail. He went on, however, building up more and more elaborate imaginings until finally the weight of his own system brought him to revision. After revision he began in new ways to make the world conform to his inner desires. By a long series of trials he arrived at methods which are fairly successful. The period of mere subjective imaginings was thus justified by later periods of more useful readjustments in consciousness. The imaged readjustments are now capable of being employed with success in bringing the outer world into conformity with the inner world.

Consciousness is seen in such examples as these to be a mediating function in which the opposition between environ-

mental forces and personal needs is overcome through a dual process of taking the environment into the individual and there working it over and over until it assumes a form in which the environment may be used to the advantage of the individual while at the same time the individual conforms to the requirements of the environment which he absorbed.

The importance of consciousness in the evolutionary processes is that it solves the age-long opposition of individual and environment in a new way, giving to both a unique recognition and to the individual a supremacy over external conditions which none of his other functions ever permitted. Contrast consciousness with the nutritive function. Through digestion much material is taken into the organism and is used to build up animal tissue, but there is no reflex influence on the outer world. The environment is not made, through the process of digestion, more digestible for the future. When, however, we discover through consciousness how to use the world for the ends of individual life, the environment itself can be modified so that it will from that time on be different in its relation to the evolved individual.

Let us take another illustration and work it out in somewhat greater detail in order to show the applicability of this concept of consciousness in the sphere of the social sciences. Primitive man found his first tools in the rough objects of nature. The sharp thorn or bone, the heavy root or branch all offered themselves to his hand as means of better meeting the demands of life. We need not pause to discuss fully the evidence of high evolution which appears in the fact that man apprehended the value of these rough objects as the other animals had not been able to apprehend it. Man saw in his more efficient conscious world the relation of the heavy club to his enemy, the relation of the sharp thorn to the object that he would rend. This in itself was sufficient evidence that the evolution of consciousness had served an important end in the improvement of individual life. Passing over the first stages of tool-consciousness, however, let us concentrate attention on the fact that as soon as primitive man took these natural implements in his hand he began to make in his inner conscious world comparisons of the



various tools which he knew. He began to recognize a smooth handle and a rough edge as advantageous. The result was a better selection of weapons and tools and the beginnings of the art of smoothing the handles and sharpening the edges of the instrument. First nature, and then a modification of nature. The next step came when man saw that these natural forms could be improved by using more durable materials. He began to work in stone. At first he copied slavishly nature's patterns and then he made shapes that nature had suggested but never perfected. Now he began to recognize that the whole matter of available materials was worth considering. The more permanent the tool and the more plastic the material the higher the advantage to which he could attain. So he took some of the most pliable and most accessible minerals and he made of them shapes that nature had taught him at first but which he had now so far refined that the credit for the present form belonged to him rather than to the world of nature. Through this schooling of his powers he began to rise to the level where forms and materials were sufficiently mastered so that he could contemplate the underlying mechanical principles which these contrivances exhibited. The analysis of the tool now reached the point where man's working over of the situation in his world of consciousness was of infinitely greater importance for even the external world than were the original forms presented to his senses. As soon as man learned to extract the principles on which his tools were constructed he was master of the environment. He now moulded materials, following his ideas, into forms and combinations that never under any chance could have come into the world through the mere operation of physical forces. For these forms a world was necessary in which images could first be worked over into new combinations which should in turn supply the pattern for a reworking of external reality. The conquest of the environment through the organization of conscious images is here illustrated so clearly that it is difficult to see how any complete history of art or industry could be written without a thoroughgoing analysis of the conscious processes which lie back of man's efforts and explain the stages through which he passed in his conquest of his environment.

Thus far I have emphasized the importance of consciousness as a means toward the end of conquering the environment. There is however another phase of the matter which calls for our attention before we shall have a complete account of the significance of consciousness for the explanation of human life. There are certain human functions which grow up as supports to consciousness. These functions are not directly related to the physical environment and would never have been perfected at a level of life where mere preservation of individual existence is the chief end of animal endeavor. These supporting or secondary functions serve the purpose of self-preservation only indirectly through consciousness. Chief among such functions is language.

Thanks to the recent studies of Wundt and others we know much regarding the functions of language. It originates as a mode of emotional expression, purely individualistic in its importance. Gradually it takes on through imitation a social character, and, finally, when society comes to be made up of beings capable of holding ideas in consciousness, language becomes a means of refining and exchanging ideas. Language never was a useful function in the direct struggle with the physical world. The man who can shout the loudest is in no wise thereby aided in enduring the hardships of cold and privation. Shouting is useful for a totally different type of adaptation. The shouter is a very valuable link in social adaptations and social adaptations are valuable in that they refine consciousness and make for more elaborate organizations of the human forces which shall conquer nature. This is what was meant when it was pointed out a few moments ago that language is a secondary or indirect factor in the struggle for existence.

When language is evolved as a secondary function supporting consciousness in its operations, there arises a new realm of fact. I know of no more vivid way of putting the matter than to say that man lives primarily in the world of words. In this world of words he carries out most of his adjustments. He feels the force of the physical environment now and then when he comes into contact with its harsh demands, but for the most part he works over and over with all his energies words and conscious relations.

If we add to our consideration of oral language the consideration of other devices such as writing and coins and bills of exchange whereby we support conscious operations as they deal with the world of physical facts at long range, we see how man has built himself a special world in which he moves. This special world is the most unique product of evolution and it is also the most effective device which has ever been produced for subjugating the physical environment to human needs. How any student of the world of human life could be content to study this life by means of a formula borrowed from the realm of animal evolution, passes my understanding. Man lives in a world of language, of indirect conscious modes of attack upon his physical environment. Man spends his chief energies developing indirect methods of attacking nature. He no longer cultivates new strength with which to pass through floods. He develops rather a science of engineering and indirect mechanical devices which shall raise him into a world where there are no floods. And yet we find our students of human life solemnly talking about the biological conception of society and the parallelism between society and the lower organisms. The fact is that the science of human life needs a formula derived from a study of the relation of consciousness to the struggle for self-sufficiency.

How completely the evolution of consciousness has removed human life from the level of animal modes of contact with the world is seen by the contemplation of human art. We derive from art the kind of satisfaction which comes from catching a glimpse into the conscious life of a fellow being. Art carries over from man to man the inner possibilities of rearranging the physical environment. A painting for example lets us see how the artist selected from the images offered to him by the outer world, and how he grouped these images for the purposes of his own conscious satisfaction. A painting is of value as a means of arousing our powers of conscious rearrangement of the world. Art is, from the purely biological point of view, of no immediate adaptive value, and yet it is recognized as one of the highest achievements of human life. What is needed here is just such an extension of the biological formula as we have all along been

suggesting. Organic evolution operated to establish self-sufficiency in the animal until finally an animal was produced in whom inner conscious processes outweighed all others in importance. These inner processes are important because they make possible the most complete readjustment of the environment. The inner processes are also highly significant because they give rise to a new world, the world of language and art which supports and fosters the further evolution of consciousness until there is established through consciousness a new system of life.

Perhaps the view which I have been defending can be made clearer by contrasting it sharply with such a view as that recently expressed by MacDougall in his *Social Psychology*. MacDougall it will be remembered deplors the strongly intellectualistic trend of our psychology and finds in this trend the explanation of the fact that the social sciences have not been able to use psychology as they should. The social sciences deal with men in action and the springs of action are the instincts. Emotion rather than cognition is the conscious accompaniment of instinctive behavior; let us accordingly rewrite our psychologies so that the students of social science may use them. In this rewritten form let us treat the instincts and the emotions as the chief factors in determining human evolution. I am sure I have not misrepresented MacDougall in this summary for in common with the rest of you I have been through the dreary pages of description in which he discusses the various emotions, and I have tried in vain to find in those pages the principles which would explain human civilization. The fact is that human civilization has not been toward instincts and emotions, but away from them. Language has evolved out of instinct, if you please, but it is so far from instinct in its present character that it has taken long generations of acute scholarship to show the process of ascent. Language is as intellectualistic a function as can be found in the world. Again take our material arts. Are buildings the outgrowths of the instinct for warmth or are manufacturing plants the products of hunger? Is commerce the outcome of the instinct of acquisitiveness? I think MacDougall's diagnosis of the difficulty with the social sciences

is better as he writes it in his preface and introduction than as he writes it in the body of his book. I agree with him when he holds that human life can be explained only by psychological principles. I do not agree with him when he minimizes intellect as distinguished from instinct. I do not sympathize with him at all in his attempt to bring human action back to the fundamental formula of all animal behavior. Human behavior is not aimed at maintaining oneself within the environment, it is aimed rather at complete remoulding of the whole environment, and the chief instrument in this process of remoulding is intellectual comparison and deliberation, not emotion. Note that I do not deny for a moment that human life is full of exhibitions of instinct and emotion. I do not deny that civilization repeatedly comes back to animal life instincts, but I find in these lapses into instinctive behavior only the necessary background of human evolution, not its typical modes of progress. Whoever would understand our buildings and our commerce and our language and our arts must study human intellect rather than human emotions.

A second view which I shall touch upon is that of Darwin. Darwin evidently felt, as did his critics, that the formula of natural selection is not adequate to explain human life. Man has not only been selected as superior in his equipment for the race of life, but he has been set apart as different. So Darwin took up the task of showing how this difference came about. He noted that many animals develop through sexual selection, powers which are ornamental and socially useful but would never be preserved merely through contact with the physical world. So he wrote his *Descent of Man* to show the importance of sexual selection. I think no reader of Darwin's writings ever feels that the author himself was enthusiastic regarding this solution of his problem. Human sexual selection could hardly have accounted for even the primitive feats of bravery which make up the legends of our heroes. Certainly the society of today has not grown out of the demands of sexual selection. The control of all social relations is under principles, not of sexual efficiency, but of moral and intellectual propriety. Indeed, the type of social organization which we

have is not the source of human superiority. It is rather the expression of human superiority. Man has not developed the most compact sexual society, — such organization is exhibited in high perfection even in the lower animal world. Man has made his way in the world by some means which first made him master of the environment. The conservation of property has been quite as strong and intelligent a motive in the organization of society as any motive that can be described. Darwin was undoubtedly in line with all our modern thinking when he felt the necessity of a special formula for human evolution, but he hardly satisfied the demand which he felt. The breach between animal life and human life is much too great to be spanned by any single form of selection. The fact is that the method and end and character of human life are all different from those described in any formula of organic selection.

I should not want to be misunderstood as assenting to their views, but I confess I always have sympathy with those critics of the early writers on evolution who stood according to their light for the time-honored separateness of man from the rest of the world. For generations human thought had regarded the animal world as another world, and here came men who would impose on fish, bird, mammal and man the same stamp. I believe the identification was overhasty. I believe that we have suffered in our later studies of man through a shortsightedness born of the biological discovery that our antecedents are those in which consciousness played but a small part. I believe we need to work further on this problem of evolution until we see that in its consummation organic evolution passes into a form of adjustment in which the inner world with its conscious pattern for changes in the outer world is more important than any form of objective selection which can be discovered.

The evidences which I have presented up to this point justify, I believe, the conclusion that consciousness continues and carries to a higher level the process of differentiation of the individual from his environment which has been going on throughout all organic evolution. The examples discussed show also, I believe, that consciousness is the means of changing very materially the significance of the environment to the individual.

At all of the lower stages of life the environment is dominant and the individual survives chiefly by withdrawing into itself. With the appearance of consciousness, however, the balance is changed. The individual takes up the environment and begins to make it over so as to conform to patterns developed within. The result is the familiar fact which has often been pointed out by the anthropologists that man has never in his history undergone any changes in his bodily organs which would in the slightest measure justify us in attempting to explain in organic terms the recent or even the remoter advances in civilization.

These conclusions are in no wise jeopardized by the difficulties which arise the moment we ask how consciousness operates in detail in bringing about these results. Indeed, I have put off the discussion of several questions which I am sure must have arisen in your minds because I have recognized that there will be divergencies of opinion in regard to these details. I have not meant to evade the difficulties and I turn now to the examination of several of them. Let me reiterate, however, the statement that the major facts are not involved. That consciousness has furnished a turning point in evolution may be, and is, as I have shown, true, even though we may have some difficulty in making that fact cohere with our theories of consciousness. That consciousness is a factor to be recognized in the study of man's position in the animal world, that consciousness is a factor which must be studied in all the social sciences, are facts which must, indeed, be properly set forth in our psychologies, but which continue to be facts of no small moment to the world and to science even while we are rewriting our psychologies to fit these facts.

One of the first questions on which we must be clear is the question whether consciousness is a cause of events in the world. I have no slightest hesitation in taking a position on this question and perhaps it will be well to state that position first and defend it later. I hold that consciousness is a cause of events in the physical world. The difficulty which many have experienced in coming to a conclusion in this matter grows, I believe, out of a failure to grasp the significance of the evolutionary concept. I have elsewhere used the analogy of life to

clear up this difficulty. When one looks backward and asks where life came from he finds himself dealing with material particles which have come into a certain combination. To the physiological chemist this backward view is the natural one. Life is nothing but the result of the combination of carbon with oxygen, hydrogen and other elements. But now let us look forward and ask about the world of tomorrow. Tomorrow we shall find new organic combinations and these will not be due merely to the fact that carbon and hydrogen and oxygen existed today. The organic compounds of tomorrow are dependent on organic compounds of today. One important cause of tomorrow's life phenomena must be sought in the fact that life is here today. We are not here concerned in going back to first causes. Where life came from in the first instance I do not pretend to discuss. It is here now and any adequate explanation of tomorrow involves the existing life of to-day. The physiological chemist may insist on our going back to carbon and the rest and I should have no objection to going with him the full road. But if I go back with him he must come in my direction with me. Carbon enters into this and this compound and from this instant forward it has in the world a unique significance. It is a different kind of a cause from that which it was before it entered into the compound.

Let me reinforce this analogy of life, by the analogy of individuality. An individual can be resolved into a great variety of elements. The individual is what he is because of these elements. If one looks backward and asks for the cause of this individual he is taking a perfectly legitimate view of causation and is quite in his rights in saying that he has in the elements of the individual an adequate account. But again let us look forward. The individual is a center of future influences and the world takes its course tomorrow because the individual did this and that. What is there wrong about the statement that the individual causes certain results? Suppose that the student of atomic structure objected to the work of the physiological chemist on the ground that carbon is no reality but merely a compact bundle of ions. Suppose that our physiological chemist were forced to say carbon is merely a



by-product in the real process of compounding ions. Carbon is an epiphenomenon; its existence is a very doubtful scientific assumption. What if we should insist that the term carbon confuses those of us who deal with ions? We do not deny on the whole that carbon exists, but do not let it turn up in serious literature because it reminds us of the old false notions about the simplicity of the atom. Of course, our physiological chemist would resent this. Why should we who deal with individual differences and other facts regarding human life be timid in asserting our scientific rights? A human being may be a composite organism made up of senses and motor functions and what not. He may even be an unstable compound as contrasted with what we now think about carbon, but every rational consideration of society must be based on a study of individuals. We shall get nowhere in our study of society if we wait for the physiological chemists to supply us with causes of social behavior. If we cannot have their kind of causal concept we should hasten to get one of our own. Their causal concept looks backward to elements, ours should look forward from productive organizations.

I have used individuality as my stalking-horse. If individuality is admitted to be important to science I am quite content to rest the case of consciousness. Consciousness is the essential fact in human life as I have attempted to show. What man does with his environment depends upon consciousness. That phase of individuality which is important enough to change the type of evolution certainly cannot be described as non-existent or as merely resolvable into its elements.

I think the difficulty in the past has been that we have been dominated by the physicist's notion of cause. With a physicist a cause must be a center of influence equal in importance whether one looks backward or forward. Causes must therefore be constant in number like the elements of matter and the amount of energy in the world. The doctrine of evolution opens the way for a wholly different view of causation. Cause ought to be definable in terms which shall make of life, not a new force, not an increment in the energy of the world, but a sphere of existence, a center of reorganization. Certainly the world is

different because life now flourishes here. The same is true of consciousness. It is not some new fact parallel with heat and electricity and gravity. It is a new sphere of adjustments. The world moves in new lines because this new sphere has been evolved. We cannot put it out of the world because of our own confusion as to how it operates. We cannot understand society by ignoring it. We cannot protect ourselves against the charge of scientific incompleteness in our account of the world by saying that consciousness is not a cause like electricity, hence it is no cause at all. I never hear a physiologist or a biologist contending that there is no possibility of a science of consciousness without wondering how the historian of scientific myths in some future generation will be puzzled by our present-day timidity regarding the causal character of consciousness. I conclude, therefore, as I began by saying that I have never found any confusion growing out of the conception that consciousness causes changes in the world. I have, on the other hand, seen much time and energy misspent in describing consciousness in timid terms which make it difficult to show its value for social science or for any other science.

A second question carries us further into detail. How does consciousness operate in controlling bodily activity? I dare say that all of us who are engaged in the study of psychological problems would agree that the most important particular problem of today is the problem of the relation of consciousness and bodily movement. Here again I believe that the evolutionary concept is the one which will clear up many of our difficulties. Let it be remembered that for a long period organic evolution was perfecting a sensory-motor mechanism which was not dominated by any highly centralized organ such as the cerebrum. There was no higher nervous center where elaborate combinations of impulses were possible. When now the inner organization reached a stage of complexity such that inner recombinations were possible and consciousness in all of its importance appeared and began to turn the scale in the struggle with the environment, there still remained a large number of processes and organizations which belonged to the earlier simpler uncentralized stage of evolution. We are in the habit of recog-

nizing this fact in the statement that consciousness does not control all of our acts. Put in other terms this statement can be made as follows. There are many of our acts which are not dependent for their character and influence upon higher processes of comparison, memory and self-control. This internal separation of the individual into higher and lower functions has given color to all of our ideas about the nature of behavior. Every individual is trying to develop inner coherency. The individual with his different levels has been so acutely aware of his effort to work out this coherency that he has had all kinds of views with regard to the higher self and the lower self. The antithesis carried man so far that he lost sight at first of the utility of his higher self as a center of functional adaptations and he grew accustomed to describing his conscious life as pure and unrelated to practical conduct. Later he was led to change his views regarding his own nature very radically. When he saw the significance of his conscious life in the evolutionary process he began to identify all his activities with sensory-motor processes which he had all along recognized as practical and thus arrived at the grotesque conclusion that he must be treated as just like the lower animals.

We are in a fortunate period of reconstruction when the relation of consciousness to behavior is being considered on the basis of elaborate objective studies of the facts. It would not be in place here to attempt a critical review of these studies. It is enough for our present purposes to point out that the evolution of consciousness has been so intimately related to the evolution of the higher forms of behavior that the progress of human intelligence can be traced in the record of behavior. The science of psychology will not find itself until it turns away from impressions and sensory details and recognizes that the inner processes of conscious organization so transform sensory elements that there is in sensation little of value for the student of consciousness. Behavior on the other hand is the expression and end of all inner organization. To study behavior more completely is therefore the most urgent of our problems, — in a very important sense it is our chief problem. When we know the evolution of consciousness we shall find the relation between consciousness

and behavior solving itself as two phases of the same single process of adaptation.

In the third place, I believe the evolutionary principles which we have been discussing reveal the limitations of structural psychology. The structural psychologist shuts himself up within consciousness as if the conscious world had within itself its own origin and ends. He makes the enumeration of forms his only function. In assuming that consciousness is a world apart, the structural psychologist is following the natural trend of experience which, as we have seen above, tends to emphasize the separateness of consciousness from behavior. The structuralist does not see the significance of the effect which is produced in human society through the evolution of a sphere of life in which rearrangements of the world may be worked out. The structuralist makes no contribution to social science, for society is not organized on the separate elements of conscious experience, but upon the effects produced through intelligent behavior upon the environment. Consciousness as a static fact to be dissected loses its whole significance, for consciousness like life is a type of functioning. Functional psychology as contrasted with structural psychology opens up the whole world of effects, both in behavior and in transformations of the physical environment. Functional psychology makes clear the relation of consciousness to other functions. How anyone can be satisfied to enumerate sensations when the rich possibilities of explaining the relation of sensation to perception, to judgment, and to behavior are equally open problems, I confess I cannot understand. How one can talk about a sphere of psychical causality, and can see in this cut-off world a legitimate sphere for science, I am at a loss to comprehend. For my own part I am persuaded that the science of psychology will never be accepted as contributing lessons worth the attention of men until this science shows the way in which consciousness has by its evolution transformed life.

Finally, I believe that the applications of psychology to practical problems will be fully worked out only when we recognize the importance of consciousness in evolution. The relation of rationality to self-control, the relation of intellect to

the arts and industries which characterize civilization, the relation of thought to the growth of institutions, these are practical problems and at the same time psychological problems. We have been in some doubt in the past as to whether society is based on instincts or on ideas. We have talked about our institutions as intelligent, but studied them as if they were mechanical. Our whole treatment of human life has been biological rather than psychological. I believe that the period of biologizing human life is over. We shall lose none of the advantages gained from a study of reflexes and instincts if we recognize that these are primitive phases of human organization and less significant than the higher conscious phases. We shall understand the productive forms of activity better if we recognize them as related to intelligence which is the consummate product of evolution. If time permitted one could carry out this reference to applications in great detail. I have referred to the development of tools. Think of the way in which modern industry exhibits in its use of commercial paper the growth of the power of conscious abstraction. Think of how early barter exemplifies the concrete, perceptual character of savage intelligence. Think of the growth of the fine arts and note how it illustrates the growth of the power to distinguish more clearly the elements of experience and combine them into more elaborate wholes. Think of the development of science in the modern world as a sudden fruition of intelligence which had been in training for long generations. In short, take any phase of human life and see how it becomes suggestive material to the student of evolutionary psychology. Note from the other side how utterly incomplete the study of these phases of life would be and would remain with consciousness left out. I believe we are on the eve of a newer psychology than any which we have known. This new type of psychology will not be unfriendly to biology for it will study evolution, but it will not be dependent on biology for its formulas. Psychology will boldly assert its right to existence as the science which deals in a broad way with the evolutionary processes by which consciousness arose and through which the trend of life has been changed from organic adaptation to intelligent conquest.

# THE NATURE AND CAUSATION OF THE GALVANIC PHENOMENON.

BY BORIS SIDIS, M.A., PH.D., M.D. AND LOUIS NELSON, M.A., M.D.

## PART I.

### THE NATURE OF THE GALVANIC PHENOMENON.

#### I.

The purpose of our present paper is to establish rigidly the fact of the presence of galvanometric deflections under the influence of psycho-physiological processes and to investigate the nature, causation and conditions under which such deflections become manifested. It is by no means an easy matter to disentangle the conditions, physical, physiological and psychological, under which galvanic deflections appear, when an organism becomes subject to external or internal stimulations. Even when the galvanic deflections due to stimulations are established still the nature and causation seem to be beyond our grasp as the factors are numerous, the conditions complicated and the whole subject of the psycho-physiological galvanic deflections appears to be intricate and shrouded in obscurity. Investigators of the subject have declared it to be a difficult one and have been unable, except for a few conjectures, to trace scientifically by means of experimentation the cause of the 'galvanic phenomenon.' We think that our investigation will not only establish the fact of the galvanic phenomenon free from all artefacts, but will also clear the subject of all its inherent obscurities and help to disclose its nature and causation. It may be well to add that the present study is a continuation of the work carried out by Sidis and Kalmus and published in *THE PSYCHOLOGICAL REVIEW* for September and January, 1908, 1909.

#### II.

Tarchanov is regarded as one of the first investigators who discovered the interesting fact that psychic states give rise

to galvanometric deflections. According to Tarchanov<sup>1</sup> all psychic processes, sensory, emotional and even purely ideational, such as imagination and calculation, are accompanied by galvanometric variations. He observed large galvanometric deflections apparently brought about not only by sensory stimulations, actual affective states and emotions, but also by the mere memory and representation of such states. Intellectual processes, ideas, images, logical reasoning, memories are sufficient to affect the mirror-galvanometer and give rise to marked deflections. As a result of his investigation, published in a brief preliminary communication, he conjectures that the deflections may be due to secretory changes going on in the epidermis. He is inclined to think that psychic activities affect the secretions of the skin which in their turn produce the marked deflections observed in the mirror-galvanometer. Tarchanov has not followed up his preliminary communication with a detailed study of the phenomena.

Ch. Féré<sup>2</sup> may also be regarded as one of the pioneers who pointed out the presence of galvanic changes under the influence of emotional states. According to this investigator the changes are due to variations of bodily resistance; in other words, Féré seems to think that emotional states lower the electrical resistance of the body. This assumption of lowering of bodily resistance has been uncritically accepted by many investigators. It is accepted even by those who otherwise follow Tarchanov and assume the alleged factor of skin secretions. It is assumed that the galvanic deflections are due to lowering of electrical resistance through the agency of skin secretions produced by psychic activities. The cause of the phenomenon is still regarded as unknown. We shall point out that the sole cause of the obscure factor of resistance is a faulty reasoning and a deficient technique.

A number of investigators such as Sticker,<sup>3</sup> Sommer,<sup>4</sup>

<sup>1</sup> Tarchanov, 'Ueber die galvanische Erscheinungen,' *Pfl. Arch.*, 1890.

<sup>2</sup> Ch. Féré, *Comptes ren. de Soc. de Biologie*, Jan.-March, 1888.

<sup>3</sup> Sticker, 'Ueber Versuche einer objectiven Darstellung von Sensibilitäts-Störungen,' *Wien. klin. Rundschau*, 1897.

<sup>4</sup> Sommer, *Beiträge*, Wien, 1902.

Sommer and Fürstenau,<sup>1</sup> Veraguth,<sup>2</sup> Jung,<sup>3</sup> Binswanger<sup>4</sup> and others have advanced various views as to the possible causation of what has become known in psychopathological literature as the 'galvanic phenomenon.' Sticker rejects Tarchanov's hypothesis of skin effects and action of sudorific glands as the cause of the observed galvanometric deflections under the influence of psychic states. He advances the hypothesis of circulation, — the galvanic phenomenon is the effect of circulatory changes in the capillary blood vessels, changes induced by psychic states in general and by emotional states in particular. In this respect Sticker agrees with the French investigators who unhesitatingly assume the hypothesis of circulation. The galvanometric perturbations are supposed to be the effect of circulatory disturbances which somehow lower the peripheral and bodily resistance. R. Vigoroux<sup>5</sup> and later A. Vigoroux<sup>6</sup> experimenting on clinical cases reject the view that the lowering of resistance is due to skin secretions; the electrical perturbations are ascribed by them to variations of resistance of blood circulation especially of the capillary blood vessels, variations of electrical resistance in some unknown way, probably by an increase or decrease of the concentration of the blood, brought about by the influence of mental states, especially by emotions.

Recently C. G. Jung, of Zürich, and his collaborators, Peterson<sup>7</sup> and Ricksher,<sup>8</sup> have carried out a series of experiments on a number of sane and insane persons. They confirm the presence of the so-called 'galvanic phenomenon' accompanying the various mental states under observation. They find

<sup>1</sup> Sommer und Fürstenau, 'Die electrische Vorgänge,' *Kl. f. Psych. u. N. Kr.*, B. 1, H. 3, 1906.

<sup>2</sup> Veraguth, 'Das psycho-galvanische Reflex-Phaenomenon,' *Monat. für Psychiatrie und Neurologie*, B. 21, 1906.

<sup>3</sup> Jung, 'On Psychophysical Relations,' *The J. of Abn. Psych.*, Feb., 1907.

<sup>4</sup> Binswanger, 'Ueber das Verhalten des psychogalvanischen Phänomens,' *J. für Psychologie und Neurologie*, B. 10, 1908.

<sup>5</sup> R. Vigoroux, 'Sur la Résistance électrique,' *Le Progrès Medical*, Jan. 21-Feb. 4, 1888.

<sup>6</sup> A. Vigoroux, 'Etude sur la Résistance électrique,' 1890.

<sup>7</sup> Peterson and Jung, 'Psychophysical Investigations,' *Brain*, V., 30, 1907.

<sup>8</sup> Ricksher and Jung, 'Investigations on the Galvanic Phenomenon,' *The J. of Abn. Psychology*, Vol. II., 5, 1908.



galvanometric perturbations in different forms of mental states. Jung regards the galvanometer as a valuable instrument in the study, analysis and discovery of so-called 'suppressed complexes' otherwise revealed by the so-called 'psycho-analytic method.' Some of the followers of the German school hail the galvanic test as a method in the study of psychopathic diseases in general and of hysterical affections in particular. Even criminology, it is claimed, may derive some benefit from the galvanic test, inasmuch as certain classes of criminals may be detected by means of the galvanic phenomenon.

Jung and his collaborators have not contributed anything to the causation of the galvanic phenomenon, but they are inclined to accept Tarchanov's hypothesis that the galvanometric perturbations are the effect of skin secretions. According to the Zürich investigators mental activities with their accompanying affective states give rise to secretions of the sudorific glands with a consequent lowering of electrical resistance which is the cause of the observed galvanometric perturbations. This conclusion is but a plausible conjecture. They think however that it is quite probable that a number of other factors concur in the causation of the galvanic phenomenon, such as circulatory changes, changes of the central nervous system and especially changes produced by mental activities and their affective states in the sympathetic nervous system. To quote from Jung, "If one applies to a subject tactile, optic or acoustic irritations of a certain strength the galvanometer will indicate an increase in the amount of the current, *i. e.*, a lowering of the electrical resistance of the body."<sup>1</sup> In another place Jung and Peterson say "change in resistance is brought about either by saturation of the epidermis with sweat or by simple filling of the sweat-gland canals or perhaps also by an intracellular stimulation or all of these factors may be associated. The path for the centrifugal stimulation in the sweat-gland system would seem to lie in the sympathetic nervous system. These conclusions," the authors go on to say, "are based on facts at present to hand and are by no means felt as conclusive. On the contrary there are features presented which are as yet quite inexplicable as, for

<sup>1</sup> *Op. cit.*

instance, the gradual diminution of the current in long experiments to almost complete extinction, when our ordinary experience teaches that resistance should be much reduced and the passing current larger and stronger. This may possibly be due to gradual cooling of the skin in contact with the cold copper plates."<sup>1</sup> As we shall see further on these investigators are on a false track, their puzzles and contradictions can be easily solved.

Again Ricksher and Jung write: "The sweat glands seemed to have more influence than any other part in the reduction of the resistance. If the sweat glands were stimulated there would be thousands of liquid connections between the electrodes and tissues and the resistance would be much lowered. Experiments were made by placing the electrodes on different parts of the body and it was found that the reduction in resistance was most marked in those places where the sweat glands were the most numerous. It is well known that sensory stimuli and emotions influence the various organs and glands, heart, lungs, sweat glands, etc. Heat and cold also influence the phenomenon, heat causing a reduction and cold an increase in the resistance. In view of these facts the action of the sweat glands seems to be the most plausible explanation of the changes in resistance."<sup>1</sup> It will be seen from our work that the Zürich school, when discussing the causation of the 'galvanic phenomenon,' has become inextricably entangled in a maze of factors which have but an indirect relation to galvanometric deflections under investigation.

Veraguth has been working assiduously and patiently for a number of years on what he designates 'the psycho-physical galvanic reflex.' He eliminates circulation and he rightly excludes skin effects as causes of the 'reflex,' but he does not arrive at any definite conclusion as to the cause of the galvanic deflections under the influence of sensory and emotional processes. Veraguth thinks that his 'galvanic reflex' is due to variations of body-conductivity or 'Variation des Leitungswiderstandes des Körpers.' He thinks this phenomenon somewhat different from that described by Tarchanov and others. To quote from Veraguth:

<sup>1</sup> *Op. cit.*

“Das psychogalvanische Reflex-phänomen besteht in einer Intensitätsvariation eines elektrischen Stromes der bei der Versuchsanordnung mindestens teilweise aus einer körperfremden in der Stromkreis eingeschalteten Stromquelle entstammt. Es spielt deshalb bei diese Anordnung die Variation des Leitungswiderstandes des Körpers gegen diesen exogenen Strom einer Rolle bei der Variation der Stromintensitäts.

Die Variation geschieht im Sinne der Abnahme der Stromintensität wenn die V.P. im Zustand der Ruhe längere Zeit in der Stromkette eingeschaltet bleibt. Durch diese Thatsache stellt sich die ‘Ruhekurve’ im Gegensatz zu den gewöhnliche bisherige Erfahrungen über anfängliche Variationen des Körperleitungswiderstandes gegen einen durchfliessenden elektrischen Strom.

Die Variation verläuft im einen der Intensitäts-zunahme wenn die V.P. Reizen ausgesetzt wird.

Das Moment der Gefühlsbetonung allein ist es nicht das die Stärke der galv. Reaction bedingt; es kommt auch bei den höheren psychischen Reizen.

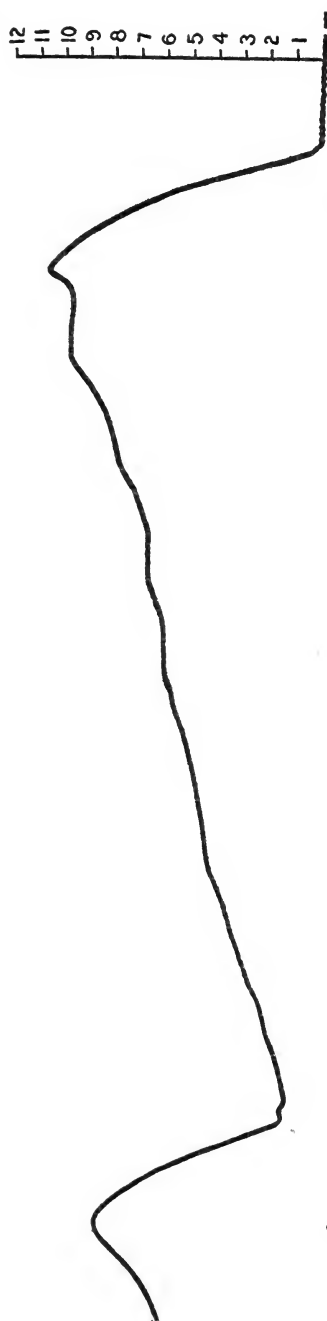
Das galv. Reflexphänomen ist also ein Indicator für Gefühlsbetonung und Actualität des psychischen Reizes.

Uncontrollierbare Variabilitäten des Widerstandes in dem Stromkreisteile ausserhalb des V.P. sind ausgeschlossen; die Tatsache der Variabilität des Leitungswiderstandes des menschlichen Körpers gegen durchfliessenden Strom ist bekannt. Mit ihr haben wir also bei unseren Experimenten mit Körperfremden durchfliessenden Strom zurechnen. Nun zeigt sich aber bezüglich dieses Leitungswiderstand ein auffalender Unterschied zwischen den obigen Resultaten und der gewöhnlichen Erfahrungen aus der Elektrodiagnostik: bei unseren Experimenten nimmt wenn keine Reize eintreten die Stromstärke ständig ab, nicht, wie wir gewohnt sind zu beobachten der Widerstand.”<sup>1</sup>

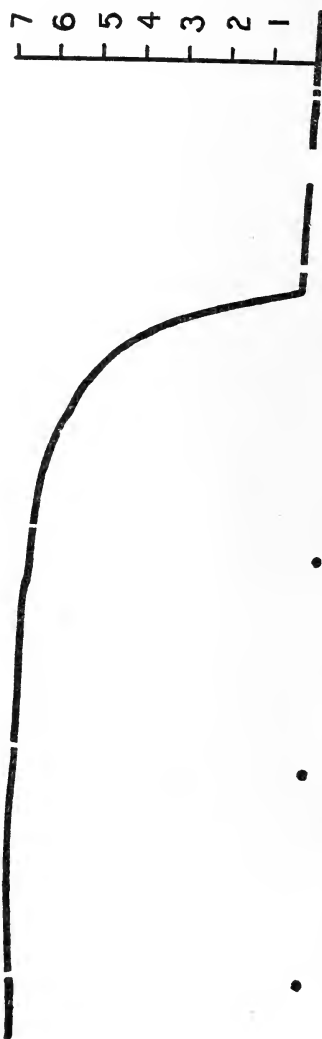
Veraguth’s ‘phenomenon’ is an artefact. The ‘Ruhe-curve’ which he regards as almost paradoxical is an artefact. The gradual diminution of the deflection, when no stimulations are given, is due to involuntary gradual relaxation of the grip on the nickel-plated electrodes used by him in his experiments. This can be shown by the following photographic curves:<sup>2</sup>

<sup>1</sup> *Op. cit.*

<sup>2</sup> The curves should be read from right to left.



CURVE I. Two cells in circuit; also shunt, nickel electrodes held by subject in each hand. Hands relax slowly. The curve slopes gradually. Intervals are indicated by the points under the curves and show minutes. Scale on curves is in centimeters; normal is zero.



CURVE II. Two cells in circuit; also shunt. Nickel-plated electrodes. When precaution is taken to have constant pressure on electrodes (no involuntary relaxation of grip) the curve is constant and shows no slope. Rhythmical breaks in the curves show minute intervals.

Sommer's attitude towards the galvanic phenomenon is rather negative. He ascribes the galvanic deflections to contact-effects between the skin and the electrodes, also to changes in the resistance of the epidermis. An involuntary increase or decrease of pressure on the electrodes would change the points of con-



CURVE III. Two cells and shunt in circuit. Electrodes put on hands passively so that the pressure of electrodes on hands was constant. There is no fall, but one continuous line with no deflections present.



CURVE IV. No cells and no shunt in circuit. The curve shows galvanometric variations due to variations of pressure and of changes of involuntary grip. The maximum variation is more than 12 cm.

tact and skin-resistance thus giving rise to galvanometric variations. It is clear that Sommer does not regard the galvanic phenomenon as the effect of processes taking place in the or-

ganism itself. The galvanic perturbations according to Sommer are rather of a purely physical character and depend on the extent of surface-contact and changes of skin-resistance. Sommer's work must certainly be taken into consideration before one can establish a definite relation between psycho-physiological processes and galvanometric deflections. The usual method of most investigators, namely, the employment of metal electrodes on which the palms of the hands rest, may lend itself to such an interpretation and therefore the galvanic reaction is really not established until that objection is obviated. Jung and Ricksher do not meet Sommer's objections when they say: "That the changes in resistance are not due to changes in contact, such as pressure on the electrodes, is shown by the fact that when the hands are immersed in water which acts as a connection to the electrodes the changes in resistance still occur. Pressure and involuntary movements give entirely different deflections than that which we are accustomed to obtain as the result of an affective stimulus."<sup>1</sup> This rejoinder is not valid as we shall see further on when we discuss the various artefacts to be avoided in order to establish the galvanic reaction.

Binswanger in his extensive study of the galvanic phenomenon does not differ in his technique from that generally employed by Jung and his collaborators with whom he also agrees in his conclusions as to the nature and causation of the galvanic phenomenon. He agrees with Tarchanov that the cause of the galvanic phenomenon is the secretions of the skin "Es scheint mir in Uebereinstimmung mit Tarchanoff und trotz der Ausführungen Stickers das es sich hier im wesentlichen um Sekretionsströme der Haut (Schweisssdrüsen) handelt."<sup>2</sup>

In a series of experiments Sidis and Kalmus<sup>3</sup> have affirmed the fact of the 'galvanic phenomenon' in relation to certain psycho-physiological states and have shown by various experiments that contact effects as well as skin changes and circulatory disturbances can be fully excluded as the causes of the phenomenon under investigation. Moreover, the same investi-

<sup>1</sup> *Op. cit.*

<sup>2</sup> *Op. cit.*

<sup>3</sup> Sidis and Kalmus, 'A Study of Galvanometric Deflections due to Psycho-physiological Processes,' *PSYCHOL. REVIEW*, Sept., 1908, Jan., 1909.

gators have demonstrated that what may be called the galvanic reaction has nothing to do with lowered resistance, whether bodily or cutaneous, produced by psycho-physiological processes; they have proven that resistance can be excluded, that the phenomenon is entirely a function of an electromotive force brought about by the action of the psycho-physiological processes set up by various external or internal sensory stimulations. To quote from the original contribution: "Our experiments go to prove that the causation of the galvanometric phenomena cannot be referred to skin resistance, nor can it be referred to variations in temperature, nor to circulatory changes with possible changes in the concentration of the body-fluids. Since the electrical resistance of a given body depends on two factors—temperature and concentration—the elimination of both factors in the present case excludes body-resistance as the cause of the deflections. Our experiments therefore prove unmistakably that the galvanic phenomena due to mental and physiological processes cannot be referred to variations in resistance, whether of skin or of body. *Resistance being excluded the galvanometric deflections can only be due to variations in the electromotive force of the body.*"<sup>1</sup> Our present work has in various ways amply corroborated the same conclusion and has definitely determined the actual cause of the observed galvanometric deflections concomitant with some psycho-physiological processes.

### III.

From the history of the subject we may now pass to a discussion of the technique of the experiments. The usual technique of most of the investigators is very simple. In connection with a D'Arsonval galvanometer one or two cells are introduced into a circuit terminating in two metal electrodes, zinc, copper, brass or steel in case of hypodermic needles. The galvanometer, being shunted, the subject places himself across the electrodes usually putting one hand palm downwards on each of the two electrodes, thus closing the circuit.

Jung describes his apparatus as follows: "The author (Dr. Veraguth) conducts a current of low tension (about two volts) through the human body, the places of entrance and exit of

<sup>1</sup> *Op. cit.*

the current being the palms. He introduces into the circuit of the current a Deprez-D'Arsonval galvanometer of high sensibility and also a shunt for lowering the oscillations of the mirror. I add to the scale a movable slide with a visiere. The slide pushed forward by the hand always follows the moving mirror reflex. To the slide is fastened a cord leading to a so-called ergograph-writer which marks the movements of the slide on a kymographic tambour fitted with endless paper upon which the curves are drawn by a pen point. For measuring the time one may use a 'Jacquet chronograph' and for indicating the moment of irritation (stimulation?) an ordinary electric marker."<sup>1</sup> In their more detailed study Jung and Peterson give the following account of the apparatus employed by them: "The mirror galvanometer of Deprez-D'Arsonval; a translucent celluloid scale divided into millimeters and centimeters with a lamp upon it; a movable indicator sliding on the scale and connected by a device of Dr. Jung with a recording pen writing upon the kymograph; a rheostat to reduce the current when necessary; and one, sometimes two, Bunsen cells. The electrodes generally used are large copper plates upon which the palms of the hands rest comfortably or upon which the soles of the feet may be placed."<sup>1</sup> Ricksher and Jung used the same apparatus with 'brass plates as electrodes upon which the test-person places his hands and completes the circuit.'

It will be observed that most of the investigators used electrodes, generally metal ones, without any precautions as to the traps encountered and to the artefacts produced. To avoid all those pitfalls and thus establish the galvanic reflex on a sure basis of facts Sidis and Kalmus employed the following technique:

"In a series with a battery was a sensitive galvanometer across which the subject placed himself, thus closing the circuit. The battery was a single cell giving a constant electromotive force of about 1 volt which was sometimes replaced by a thermoelement giving only a few millivolts, and sometimes entirely removed from the circuit. The galvanometer was of the suspended coil, D'Arsonval type and of extreme sensitiveness.

<sup>1</sup> *Op. cit.*



The deflections were read by means of a beam of light deflected from a mirror attached to the moving coil of the instrument, to a telescope with a scale. A deflection of 1 cm. on the scale corresponded to less than  $10^{-9}$  ampere through the instrument. This extreme sensitiveness was too great for many of our early experiments so that a resistance  $R$  which could be varied to reduce the sensitiveness to any desired degree, was shunted around the galvanometer.

“The electrodes were glass vessels of about 4 liters capacity nearly filled with a strong electrolyte, as for instance a concentrated solution of NaCl. Into these vessels large copper electrodes of about 500 cm.<sup>2</sup> area were permanently placed. The circuit was completed by placing the hands, feet, etc., one into each electrode solution.

“The galvanometric deflections may be due to changes in the resistance at the electrodes brought about by such purely physical causes as motion or muscular contractions of the hand, stirring of the electrode fluid or similar incidental secondary effects. In order to eliminate the possibility of such effects it was necessary to devise such electrodes that the current through the circuit should, within very wide limits, be independent of the position of the hands. The possible sources of error at this point which would change the effective surface of the hands are twofold — (1) due to the variation of the liquid level at the wrist, and (2) due to movements of the hand as a whole. The following device was used to overcome those difficulties. The wrist was covered with shellac for a length of several inches, so that the free liquid-surface of the electrode was always in contact with shellac. The shellac was covered by a layer of paraffin, though a moderate coating of shellac alone was such a good insulator that the electrode resistance became independent of the height of liquid on the wrist. In addition to this the hand was put in splints in such a manner that only a small fraction of the skin was covered, so that no appreciable muscular contraction of the phalanges could take place (the same skin-area being washed by the liquid electrodes). If now a stimulus was given which aroused an emotion or definite affective state in the subject, a marked galvanometric deflection was observed.”

After excluding resistance, both of skin and body, circulation, skin secretions Sidis and Kalmus give, as the result of their investigations, the following summary: "*Our experiments thus clearly point to the fact that active physiological, sensory and emotional processes, with the exception of ideational ones, initiated in a living organism bring about electromotive forces with consequent galvanometric deflections.*"<sup>1</sup>

In our own technique we at first closely followed that of Sidis and Kalmus with the only difference that our subjects were not human beings, but rabbits and frogs. In the course however of adaptation of the technique to the special conditions of experimentation as well as in our efforts to eliminate complicating factors and have the results free from artefacts the technique has become substantially modified. We shall give an account of these important modifications as we proceed with the exposition of the results of our investigation.

#### IV.

'Before however we give an account of our technique and its gradual modification in its adaptation to the needs of the experiments in hand, it is well to give a brief review and possibly a short discussion of the main artefacts to which this work is subject. In carrying on experiments on such an intricate problem where the factors, physical, physiological and psychological, are so numerous and complex special care must be taken to avoid the artefacts which are sure to creep in and vitiate the results. The first requirement in such work is simplification of the technique so as not to introduce conditions which are apt to complicate matters and obscure the possible solution of the problem. The conclusion arrived at by Sidis and Kalmus, differing widely from that arrived at by earlier investigators, namely, that the galvanic phenomenon is not due to resistance, whether of skin or of body, but to an electromotive force, helped us materially in the simplification of the conditions of the experiments, a simplification which those investigators have afterwards adopted in the course of their work. This simplification consists in the discarding of the electric batteries introduced into the circuit. The introduction of electric cells is apt to mis-

<sup>1</sup>*Op. cit.*

lead the investigator from the very start, inasmuch as he is unconsciously led to postulate that the resultant galvanometric deflections are due to resistance. He assumes that the only electromotive force present is the one derived from the cells and which is therefore constant. Since the strength of the current  $C$  is  $= E/R$  and as  $E$  or the E.M.F. of the cells is constant the variations of the current  $C$  which give rise to the deflections of the galvanometer must necessarily be due to variations of  $R$ , that is, of resistance. Since resistance  $R$  consists of two elements (1) resistance  $r_1$  of the physical system, cells, electrodes and galvanometer, and (2) resistance  $r_2$  of the body; since again resistance  $r_1$  is constant, it necessarily follows that the galvanometric deflections are due to variations of resistance of the elements or tissues of the body. It is this faulty technique of using cells from which the E.M.F. is supposed to be derived and passed through the body of the test-person that has given rise to the unproved assumption that the variations of the current which produce the galvanometric deflections are due to lowering of bodily or tissue-resistance.

It is clear, if we make no assumptions, that in the formula  $E/R$  the variations may take place either in  $E$ , or in  $R$ , or in both. In other words, the unbiased experimenter realizes at the start that he deals here with electromotive forces and resistances which either alone, or both, may participate in the causation of the observed galvanometric deflections. While therefore it is a fundamental fallacy, a *petitio principii* as it is termed in logic, to make at the outset the unwarranted assumption of ascribing the galvanic effects to variations of only one of the factors, namely, resistance, it is on the other hand a serious error of technique to use cells in the circuit and thus complicate unnecessarily the conditions of the experiment. The introduction of more elements, of cells and shunts, brings in more electric forces and resistances into the circuit and thus only helps to complicate and obscure the investigation of an intricate subject. We must remember that the first requirement of an experimental work is not complication, but elimination and simplification.

If we examine more closely the conditions of experimenta-

tion of the various investigators, we find that one of the most serious artefacts results from the employment of metal elec-



CURVE V.  $V$ . (a) Copper electrodes; no cells and no shunt in circuit. The electrodes were put on the hand *passively* so that there was no alteration due to *active* pressure. (b) Copper electrodes under same conditions of circuit. Hands put *actively* on the copper electrodes.  $V_{11}$ . Show passive (a) and active (b) pressure of platinum electrodes.  $V_{11}$ . Show passive (a) and active (b) pressure of tin foil electrodes.

trodes, such as copper, zinc, nickel, brass and steel in direct contact with the fluids of the palmar surfaces of the hand. Calomel-mercury electrodes present similar artefacts on account

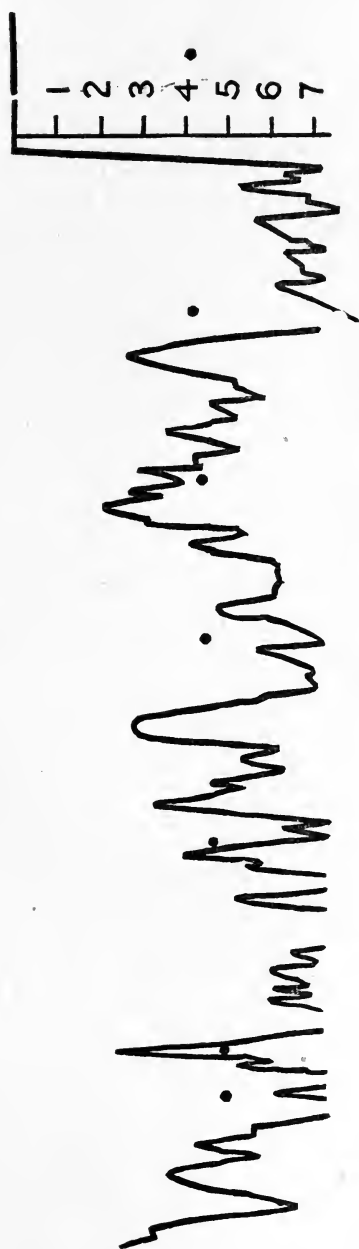
of the impurities giving rise to currents with sudden and often ceaseless fluctuations of the mirror-galvanometer thus causing extensive artefacts seriously impairing the value of the results. One cannot help realizing the full force of Sommer's objections that under such conditions numerous variations of contact are brought about, variations which in themselves are amply sufficient to account for the observed galvanometric perturbations. The results are at any rate vitiated and totally obscured. Under such conditions of experimentation galvanometric deflections cannot possibly be correlated with psycho-physiological changes. It can also be shown that in the use of metal electrodes the galvanometric deflections obtained when the hands are placed on the electrodes differ widely from those obtained when the same electrodes are put (passively) on the hands (see Curves V,  $V_1$ ,  $V_{11}$ ).

An important source of error is the employment of polarizable electrodes. The physical currents induced by polarization give rise to so many electrical variations and consequent galvanometric deflections as to destroy the scientific value of the results. The fluids of the palmar or of the skin surfaces in contact with the polarizable metal electrodes initiate a number of currents which ceaselessly give rise to large sudden deflections of the mirror-galvanometer. In cases where we have all those conditions combined, namely, increased or decreased surface-contact and pressure accompanied with changes of polarization we can realize how unreliable and untrustworthy the final results are. The current view that the galvanometric deflections are due to skin-effects is quite in accord with the artefacts of the experiments, since under such conditions the main galvanometric deflections do occur under the various influence of skin-effects. As long as the experiments are conducted under such faulty conditions and are beset with such serious artefacts not only is it vain to expect a correct view of its causation, but even the very fact of the correlation of psycho-physiological processes and galvanometric deflections cannot be established with any degree of certainty. The claim of Jung and his collaborators that "when the hands are immersed in water which acts as a connection the changes still occur" is in itself beset with many errors. In the first place, if the liquid is put in two different

vessels, the liquid must be of the same concentration and of the same temperature, otherwise we get deflections due to difference of temperature and concentration; then again the least change of level of the liquid will change the level at which the electrodes are washed which will produce new currents. At the same time the change of level of the liquid will change the area of the skin washed and will once more initiate currents. Then, again, the wires and metal plates become polarized and additional currents supervene. The artefacts are here so numerous that to obtain any results is almost hopeless. Sidis and Kalmus who worked with liquid electrodes had to contend with all those difficulties and could only circumvent and overcome them with constant vigilance for artefacts and painstaking precautions such as the careful use of pure or distilled water of the same temperature, the use of shellac and paraffin as well as splints for the hands. The following photographic curves will give one a clear idea of the ceaseless play of currents and hence of the artefacts met with in the use of polarizable metal

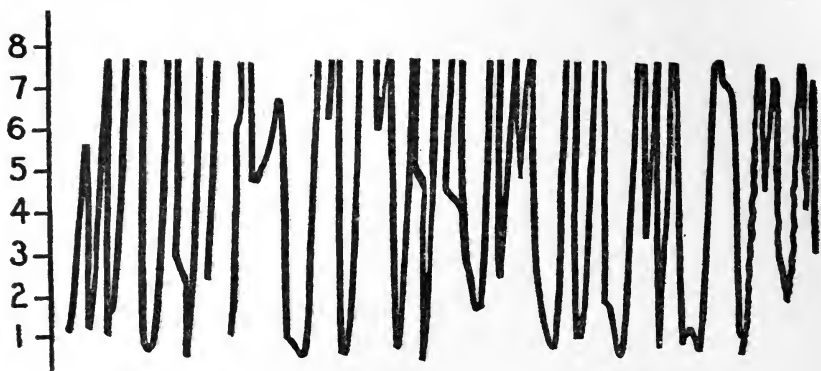


CURVE VI. Brass electrodes without cells. Electrodes held in the hands. Maximum deflection is more than 14 cm.



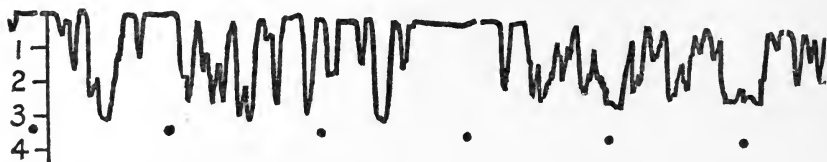
CURVE VII. Copper electrodes (plates) in two vessels filled with water. Hands inserted into the vessels. Two cells and shunt in circuit.

electrodes or of liquid electrodes when the necessary precautions are not taken.



CURVE VIII. Same condition of liquid electrodes but without cells and shunt in circuit. Disturbances of level of liquid and slight restlessness of hands. Maximum deflection is more than 8 cm.

When working with hypodermic electrodes the effects of polarization should be specially taken into consideration. Steel or iron being impure becomes easily affected chemically, thus giving rise to currents with large variable galvanometric deflections. The following photographic curves obtained with steel electrodes inserted into the skin of a rabbit's abdomen bring out clearly the effects of polarization :



CURVE IX. Steel electrodes inserted under skin of abdomen of rabbit. No cells, no shunt. Maximum deflection about 3 cm.

It becomes clear from what we have said how cautious one has to be and with what numerous difficulties one has to cope in the investigation of this subject. Considering then the difficulties and numerous artefacts we had to contend with it may be of interest to point out the development of our technique. As we have already mentioned the fact our work links on to that of Sidis and Kalmus and naturally our technique was the same as theirs. These investigators started with the usual pro-



cedure, common to all earlier investigators, of introducing cells into the circuit. Unlike however other investigators they did not start with the tacit assumption of regarding the observed galvanometric deflections as due to variations of the factor of resistance alone. They were on the lookout for variations both of electromotive forces and resistance. Since the trend of their experimentation was clearly in the direction of electromotive forces and towards the total elimination of resistance as a factor in the galvanic perturbations, they finally in their later experiments completely dispensed with the cells and superimposed electromotive forces derived from outside sources and worked with the electromotive forces manifested by the organism under the influence of external stimulations. Such a procedure is essential as it deals directly with the phenomena under investigation. We followed the same procedure and discarded the cells. The physical system of the circuit was thus greatly simplified. Moreover, we decided to work on animals instead of test-persons who are by no means favorable subjects for experimentation. Animals present us with great scope for experimentation, for surgical operations, for the injection of various drugs and thus afford opportunities for the study of the causation of the galvanic phenomenon and allow the exclusion of the various complicating factors not concerned in its production.

Now in our experiments with animals, rabbits and frogs, we found that the technique of Sidis and Kalmus had to be further modified. In the first place the liquid electrodes with shellac, paraffin and splints proved inadequate as the hairy legs of the rabbit did not quite lend themselves to such manipulations. Shaving the hair was not satisfactory, taking it off chemically produced an undesirable inflammation unfavorable to the purpose of our experiments. Besides, the liquid-electrodes proved unsatisfactory as it was difficult to restrain the rabbit from agitating the liquid and sometimes spilling the contents of the vessels, thus changing the levels of the liquid with its consequent large galvanometric deflections. The technique was defective, because we had not only to watch the deflections, but also the rabbit, the fluid, the vessels and the wires. Another objection to liquid-electrodes is the fact that they do not elimi-

nate skin-effects which, as it has been demonstrated, are not concerned in the causation of the galvanic reaction. We were therefore forced to give up liquid-electrodes and in order to eliminate the skin, we had to fall back on hypodermic electrodes. This procedure not only considerably simplified the conditions of experimentation, but it also, at one stroke, so to say, greatly simplified our problem, since we thus got rid of the factors of pressure, increased and decreased contact-area and of all the disturbances that might be ascribed to the action of the sudorific or sebaceous glands. The simultaneous simplification of method and problem was too important not to take advantage of. Hypodermic electrodes were clearly indicated by the conditions and nature of our work.

It is however one thing to find that hypodermic electrodes are indicated and it is quite another matter to find the proper kind of electrodes. We found that copper, iron, steel, nickel, brass, had to be rejected, because of the ease of polarization giving rise to variable currents with consequent variations of galvanometric deflections (see Curves VI., VII., VIII.).

It was found that platinum is sufficiently pure so as not to become polarized and is therefore well adapted to our purpose. When using hypodermic platinum-electrodes, the galvanometer was found to remain steady as can be seen from the following photographic curve:



CURVE X. Platinum electrodes in abdomen of rabbit. No stimulation; rabbit quiet. No cells: no shunt.

This steadiness of the galvanometer is of the utmost importance, because it gives us a *steady* zero-reading, while in the case of other investigators there is no steady zero-reading, since their galvanometer keeps on ceaselessly varying, thus making the results uncertain and even destroying their value.

Platinum hypodermic electrodes were used by us throughout our work. Our technique thus far was extremely simple: a D'Arsonval type of galvanometer with scale divided into milli-

meters, platinum hypodermic electrodes and a key for closing and opening the circuit.

Focal distance of mirror to lamp is one meter.

Sensibility is 225 megohms.

Period is 9.5 seconds.

The sensibility is given in the number of megohms resistance through which one volt will give a deflection of one millimeter at one meter distance. The period is the time of swing from the maximum deflection to zero.

We found it requisite to take photographic records of the galvanometric deflections. We shall give a detailed description of the apparatus and its complete outfit in its proper place.

## V.

The animal was put on an animal board and kept quiet, while the hypodermic electrodes were inserted into the body, usually well under the skin or through a muscle. We may now pass to the experiments. We quote a few experiments selected from our laboratory notes:

*Experiment I.* — Live rabbit; hypodermic platinum-electrodes inside of thigh.

	cm.
Galvanometric zero reading before closure of circuit.....	24
Galvanometric reading after closure of circuit.....	27
Deflection gradually diminishes and in 4.5 minutes returns to	24
Circuit open.....	24
Circuit closed.....	24

Opening and closing the circuit did not change the galvanometric zero reading.

	cm.
Circuit closed, galvanometric zero reading.....	24
Stimulus, pinch, galvanometric reading.....	23.95
	23.90
	23.85
	23.80
Galvanometer returns to.....	23.85
	23.90
	23.95
	24

*Experiment II.* — Same live rabbit; hypodermic platinum-electrodes inside of forelegs.

	cm.
Galvanometric zero reading before closure.....	24
After insertion of electrodes and closure of circuit gal-	
vanometric reading.....	27
After a period of 4 minutes galvanometric reading.....	24
Galvanometer stationary at.....	24
Stimulus, sharp snap on nose ; galvanometric deflection	24.10
	24.20
	24.30
	24.40
	24.50
Galvanometer returns to.....	24.40
	24.30
	24.20
	24.10
	24

### *Experiment III.*

	cm.
Galvanometric reading ; circuit open.....	24
Galvanometric reading ; circuit closed.....	24
Stimulus, series of sharp snaps on nose ; galvanometric	
reading .....	24.10
	24.20
	24.30
	24.40
	24.80

Galvanometer then returned to its original zero reading.

	cm.
Another series of sharp snaps given to the nose after rabbit	
had rest brought galvanometer reading to.....	24.80
A further series of snaps did not increase galvanometer	
deflection ; galvanometer reading remained stationary at	24.80
After a few minutes galvanometer returned to original	
zero.....	24

*Experiment IV.* — New fresh rabbit. Hypodermic electrodes inserted in forelegs.

	cm.
Zero reading ; circuit open.....	24
Galvanometric reading ; circuit closed went up to.....	27
Galvanometer returned and remained stationary at .....	24
Stimulus, prick ; galvanometric deflection.....	24.10
	24.20
	24.30
	24.40
	24.50
	24.60
	25

Galvanometer gradually returns to original zero.....	24
A series of prick-stimuli given immediately after gave	
galvanometer deflection.....	24.10
	24.20
	24.30
More stimulation gave no further deflection.	
Galvanometer gradually returns to .....	24

We must mention here one important point. Every time the platinum electrodes were taken out to be inserted again, whether in a new fresh animal or into the same animal, they were sterilized on a flame and thus purified from extraneous matter. This was the procedure in all our experiments.

To return to our work :

*Experiment V.* — New fresh, live rabbit.

	cm.
Circuit open ; galvanometric reading.....	24
Circuit closed ; galvanometric reading .....	27
After a few minutes galvanometric reading.....	24
Ammonia applied to rabbit.....	24.10
(Rabbit moved slightly.)	{ 24.20
	{ 24.30
Galvanometer returns to.....	24
Ether given to rabbit.....	24.10
	24.20
	24.30
	24.40
	24.50
Galvanometer returns to.....	24
Ether continued.....	24.10
	24.20
	24.30
	24.40
	24.50
Galvanometer began to return to .....	24.40
	24.30
At this stage rabbit moved, galvanometric deflection.....	24.40
	24.50
	24.60
	24.70
Galvanometer returned to.....	24.20
Rabbit moved, galvanometric deflection to.....	24.50
Galvanometer returns to.....	24
Everytime rabbit moves; galvanometric deflection rises to	24.50
Then returns to.....	24
Rabbit moves again ; galvanometric deflection to.....	23.50
Rabbit quiet ; galvanometer returns to.....	24

Rabbit completely narcotized; galvanometric reading...	24
Stimulations, such as pricks, snaps, ammonia produce no effect; galvanometer remains unaltered; galvanometer at zero reading.....	24

When rabbit came out of narcotic state the galvanometric deflections under various stimulations were the same as before narcotization :

	cm.
Galvanometric zero-reading, circuit open.....	24
Galvanometric zero-reading, circuit closed.....	24
Stimulus, prick.....	24.10
	24.20
	24.30
	24.40
Galvanometer then returned to.....	24

Out of the many experiments carried out on frogs we take one series as typical of many others.

*Experiment VI.* — Live frog. Platinum electrodes inserted into each thigh.

	cm.
Circuit open, galvanometric reading.....	24
Circuit closed, galvanometric reading.....	24.40
After a few minutes, galvanometric reading.....	24
Closed and opened circuit several times, galvanometer at	24
Abdomen hit a few times, galvanometer reading to.....	24.05
	24.10
	24.15
	24.20
Galvanometer then returned to zero-reading.....	24
Frog struggled; galvanometric deflection to.....	23.90
	23.80
	23.70
	23.60
Galvanometer then returned to.....	24
Hitting abdomen sharply a few times; galvanometer reading to.....	25
Galvanometer returned to.....	24
Burn (frog struggled); galvanometer reading to.....	23.60
Returned to.....	24
Acetic acid (stimulus); galvanometer reading to.....	24.50
Returned to.....	24
Alcohol injected into mouth of frog; galvanometer reading to.....	23.90
	23.80
	23.70
Galvanometer returned to.....	24

*Experiment VII.*

	cm.
Strychnine injected into lymph-sac of frog; from zero	
reading .....	24
	24.05
	24.10
Galvanometer returned to.....	24
	cm.
Galvanometer at.....	24
Stimulus, pinch leg.....	23.95
	23.90
	23.85
	23.80
Galvanometer returned to.....	24
	cm.
Galvanometer at.....	24
Stimulus pinch (frog struggled violently).....	25
	26
	27
	28
	29
	30
	31
	31.50
Galvanometer gradually returned to .....	24

After strychnine took effect stimulation began to give large galvanometric deflections.

	cm.
Stimulus, tapping abdomen slightly, from zero.....	24
Galvanometric reading.....	24.90
	24.80
	24.70
	26.50

Even tapping the board produced deflections ranging from 24 to 23.70, to 24.20 and back to 24 cm.

	cm.
Tapping the abdomen of frog sharply, from zero-reading	24
Galvanometric deflection.....	23
	22
	21
	20
	19
	18.90
Galvanometer back to .....	24

Frog in convulsions; galvanometer keeps on oscillating from zero-reading 24 cm. to 23.80, to 24, 24.10, to 24.20 and again to 24 cm.

The summary of our experiments with various frogs runs in our note-book as follows; "Frog motionless on board, no deflection. Every time frog moves, galvanometric deflection observed. The extent of the deflection appears to be proportionate to the amount of movement. Alcohol poured on the head of the frog; reaction violent, movements very extensive, large galvanometric deflections. Strychnine 3 drops administered to frog hypodermically. At first frog was quiet, no galvanometric perturbations. Afterwards frog in convulsions, galvanometric deflections amount to 10 centimeters."

The experiments in both species of animals, rabbits and frogs, give us practically the same results. Of course, we should expect to find that in animals so widely different as the rabbit and the frog the extent of the galvanometric deflections would differ under the influence of external stimulations.

At this stage of our work the experiments prove conclusively the following propositions:

1. Every sensory stimulation is accompanied by a corresponding galvanometric deflection.
2. Motor reactions intensify the galvanic phenomenon giving rise to a more extensive deflection.
3. Motor activity is by itself sufficient to give rise to large galvanometric variations, as found in the rabbit and more especially in the frog poisoned by strychnine.
4. The hypodermic electrodes, excluding the effects of epidermis, show that the galvanic perturbations due to external sensory stimulations are not the resultant of skin-effects. In other words, the skin is not concerned in the manifestation of the 'galvanic reaction.'

This conclusion will be established more rigidly by a different set of experiments.

The galvanic reaction being established by our experiments the question may be raised as to whether our experiments give us an insight into the nature of the galvanic phenomenon. Are the galvanometric deflections correlative with psycho-physiological changes induced by external sensory stimulations due to variations of resistance, lowered resistance, or are the deflections due to an electromotive force initiated in the organism



itself by the action of psycho-physiological processes? We may say that our experiments prove conclusively that the galvanic phenomenon is not due to changes of electrical resistance, but to the action of a newly generated electromotive force.

If we scrutinize our experiments more closely, we find that, when the circuit is open, that is, when there is no current, the galvanometric zero-reading is 24 cm. On the insertion of the hypodermic platinum electrodes and closure of circuit there is an initial galvanometric deflection which indicates the presence of a current. This current is due to the slight injury of the tissues produced by the insertion of the electrodes and also due to difference in temperature. After a period of four or five minutes the current subsides and the galvanometer returns to its original zero-reading when the circuit is open and no current is flowing through the system. If we now open and close the circuit, the galvanometric reading remains unchanged at the original zero-reading. In other words, there is no current on opening or closing of circuit. If now, with circuit closed and galvanometric reading at its zero-reading, we prick, pinch, burn, or stimulate the animal in various other ways, we get a galvanometric deflection which can only be brought about by the generation of an electromotive force. It is clear that no change of resistance without an electromotive force can possibly bring about a galvanometric deflection. *Hence our experiments prove conclusively that the galvanometric deflections are not due to changes of resistance, but to electromotive forces.* Since the hypodermic platinum electrodes exclude the effects of contact, pressure and skin, it is obvious that *the galvanic phenomenon can only be due to an electromotive force initiated in the organism itself by the psycho-physiological processes under the influence of external stimulations.*

## PART II.

### THE CAUSATION OF THE GALVANIC PHENOMENON.

#### VI.

The problem of the causation of the galvanic phenomenon is highly complex. The physiological processes concerned in the phenomenon may be secretory, coming from skin or other

glandular organs; may be circulatory, due to the blood vessels or lymphatics; may be due to intestinal changes, such as peristalsis; may be nervous, due to the action of the central or sympathetic nervous system, or may be due to tissue-metabolism and activities taking place in the organism, or all of them may participate in the production of the galvanometric deflections brought about by various sensory stimulations. It is by no means easy to disentangle such an intricate mesh of factors. At one stage in our work the experiments seemed to indicate as if peristalsis, with its complex metabolic processes, were concerned in the phenomenon. Thus when the hypodermic platinum electrodes were inserted into the legs or the chest, the initial maximum deflection was about 3 centimeters, the initial deflection when electrodes were inserted in the abdomen was far larger, often amounting to more than 50 centimeters, the ray occasionally getting off the scale. Moreover, constant rapid galvanometric oscillations were present, oscillations not observed when electrodes were inserted in any other place than the abdomen. To give a few of our experiments:

*Experiment I.* — Live rabbit.

Galvanometer zero, circuit open.....	cm. 24
Platinum electrodes in abdomen. Galvanometric deflection off scale returned to 50 and then gradually returning to .....	24
Deflection to 17; oscillating between 16 and 17.	

*Experiment II.* — Live rabbit.

Circuit open, galvanometer zero.....	cm. 24
Platinum electrodes in abdomen; circuit closed, off scale, galvanometer went to 3, returning rapidly to 7, 8, 9, 10, 11, to 16, 17, and kept on oscillating between 16 and 17.	

*Experiment III.* — Live rabbit (new).

Circuit open, galvanometer zero.....	cm. 24
Platinum electrodes in abdomen. Circuit closed; deflection to 4 cm., then gradually returns to original galvanometer zero.....	24

Galvanometer keeps on oscillating between 24 and 25 with an occasional large deflection of more than 50 centimeters ascribed to a possible 'rapid transit' of food in the intestinal tract.

An autopsy, however, of the rabbit showed that the intestinal tract was injured in many places by the electrodes giving rise to a number of points of hemorrhage. The results therefore were pure artefacts produced by demarcation-currents or currents of injury having little or nothing to do with the galvanic phenomenon.

*Experiment IV.* — Live rabbit.

Galvanometer zero..... cm.  
24

Platinum electrodes in abdomen, electrodes put so that they should produce no scratches, perforations and points of hemorrhage. Under such conditions the galvanometric deflection when circuit is closed: 26-24.50-24 cm. One fact, however, was of great interest from our standpoint and that was the relatively larger extent of the deflection produced by various stimulations and motor activities of the rabbit, the deflections varying from 4 to 10 and even to 20 centimeters.

We then decided to open the abdomen and find whether there could be directly observed any relation between peristalsis and galvanometric deflections.

*Experiment V.* — Rabbit given three grms. of urethane. Abdomen opened; intestine exposed; rabbit put into bath of 0.8 per cent. sodium chloride solution.

Circuit open, galvanometer zero..... 24 cm.

Platinum electrodes in sides of abdominal cavity and when circuit closed galvanometric reading 15, 20, 21, 22, 23, 24.

Rabbit struggles; galvanometer 20 and then off scale.

Galvanometer then returns to its original zero-reading, 24 cm.

Galvanometer keeps on oscillating from 24 to 25-25.50-26.

*Experiment VI.* — After 24 hours dead rabbit in same bath. The oscillation is of the same magnitude from 1-2 centimeters. Platinum electrodes taken out of the rabbit and put in the salt solution alone; the galvanometric deflections were observed to be of the same magnitude, of 2 cm. It was evident that the deflections and oscillations were due solely to the chemical processes and electrical currents generated by them. To clinch the proof the rabbit was taken out of bath and washed with clean water and then electrodes inserted into the abdominal cavity. No further changes were observed. The oscillations

then were artefacts and could certainly not be ascribed to the action of peristalsis. We were then on the wrong track. Still the fact remained that with the electrodes in the abdomen and with all the precautions against injuries and scratches which the autopsies of the rabbits showed to be absent there were undoubtedly relatively far larger deflections than when the electrodes were placed in any other part of the body.

The large abdominal galvanometric deflections which sometimes occur so sporadically gave us good cause to think that we may be here on the track of some of the important factors concerned in the causation of the galvanic phenomenon and that could only be accomplished by a more perfect method of recording the results of our experimentation. What we needed was a record of all the galvanometric deflections that had taken place, — to get, so to say, a continued history of all the changes that had taken place during a certain period. In short, what is requisite is a graphic method and the best graphic method is to get a photographic record which has the advantage of being trustworthy, automatic and continuous. Not only should the photographic records give good continuous curves, but the curves should be for long periods. The apparatus should give us a continuous photographic record at least for a period of two hours. At the same time there should be a chronograph marking time and a marker indicating any important change or time of stimulation. The following is a description of the apparatus used:

The apparatus consists of a Ludwig kymograph  $K$  to which is attached a system of two drums  $D$ ,  $D_1$  by means of two pulleys  $P$ ,  $P_1$  and belt  $H$ . Around the belt there is wound a belt of paper to which a length of six feet of photographic paper may be attached. The galvanometer  $G$  is placed on a solid table built to the wall, so that no vibrations should affect it. The source of light is  $L$ , a Nernst lamp, which is well covered by a box having a very small narrow vertical slit in it. The pencil of rays coming from the narrow vertical slit is reflected in the mirror of the galvanometer which is placed in the focal distance from a screen  $S$  with a horizontal slit which reduces the reflected rays coming from the galvanometer to a point of light. This

ray of light passing through the horizontal slit of the screen falls on the sensitive paper  $H$  attached to the belt of paper around the two drums.

For recording the time there is a time-marking device  $C_1C_2C_3S_1$  which consists of an ordinary alarm clock with a

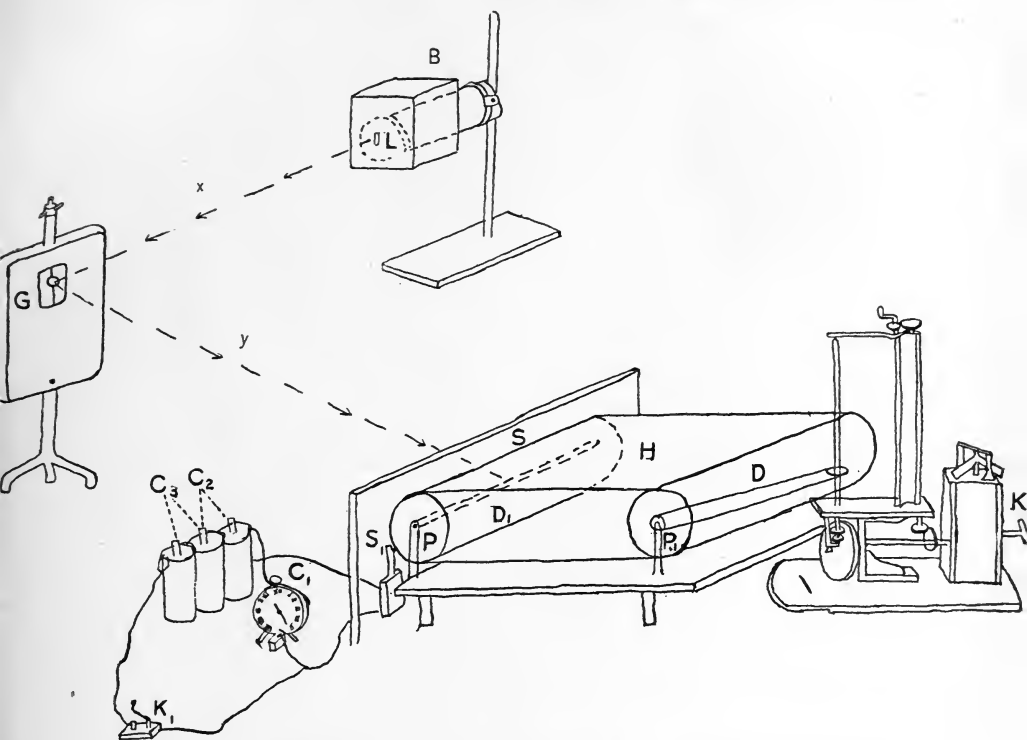


FIG. 1.  $L$ , Nernst light;  $B$ , box covering  $L$ ;  $G$ , galvanometer;  $x$ ,  $y$ , path of light;  $S$ , board with horizontal slit;  $D$ ,  $D_1$ , drums;  $S_1$ , telegraphic sounder;  $C_1$ ,  $C_2$ ,  $C_3$ , clock and cells;  $K_1$ , key;  $H$ , belt of paper;  $P$ ,  $P_1$ , pulleys;  $K$ , kymograph.

prolonged second hand dipping every minute into a cup of mercury thus closing a current coming from three cells. This current is transmitted to a telegraph-sounder  $S_1$  which marks the time on the sensitive paper. By means of a key  $K_1$  the same current is shunted and used to indicate on the revolving sensitive paper the time of stimulation or any other important event taking place during the experiment.

The apparatus fulfilled all the conditions outlined above, —

it gave a long and continuous record of the history of the galvanic happenings. As the photographic record was registered automatically on the sensitive paper we could turn our attention to the rabbit and watch closely any disturbances in the animal. Since the light reflex moving on the brass slit could be easily noticed in the darkened room even from a distance of several meters, it was an easy matter to watch the disturbances taking place in the animal as well as any galvanometric perturbations occurring simultaneously. The time of the disturbances was automatically recorded by the marker on the sensitive paper. The marking of the stimulations and of the changes in the animal placed each event, as it occurred, in its proper position with regard to the galvanic curve. This enabled us to correlate at a glance the disturbances in the animal with the corresponding galvanometric deflections. Armed with this technique we returned with a renewed vigor to the attack of the problem of the causation of the galvanic reaction due to psycho-physiological processes.

In order to come somewhat more closely to the main factor concerned in the production of the galvanic phenomenon it was thought that it might be well to approach the problem by subjecting to test sensitivity itself, especially the affective or the algedonic tone of it which has been demonstrated to be somehow related to the galvanic phenomenon under investigation. In modifying the sensitivity it was hoped that we might possibly be enabled to observe the simultaneous variations of some other factor more closely connected with the various changes of the galvanic reaction.

If again peristalsis is somehow concerned in the causation of the galvanic phenomenon, the modifications of peristalsis should also affect the galvanometric perturbations or possibly that factor which is directly concerned in the causation of the galvanometric deflections.

The modifications of sensitivity, gradual decrease and even total annihilation of sensitivity and then again its gradual increase, are brought about by various anæsthetics, especially by ether and chloroform, while the modifications of peristalsis can be brought about by various purgatives such as magnesium sulphate, *oleum ricini*, *oleum tiglii*, etc.



CURVE XI. Platinum electrodes in abdomen of rabbit. First part of curve shows normal, then ether given. Marked deflection during struggle of animal. Rest of curve without deflections, result of anaesthesia. Maximum deflection is about 5 cm. No cells : no shunt.



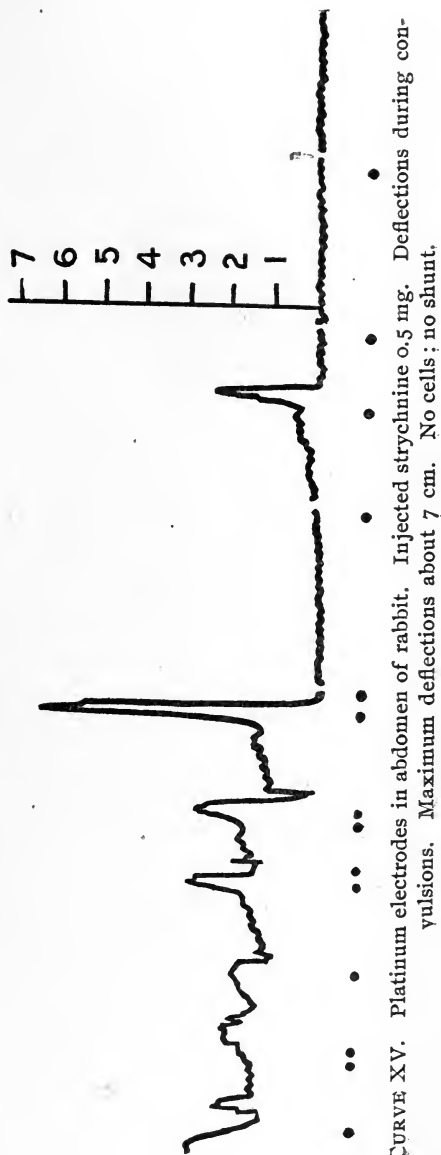
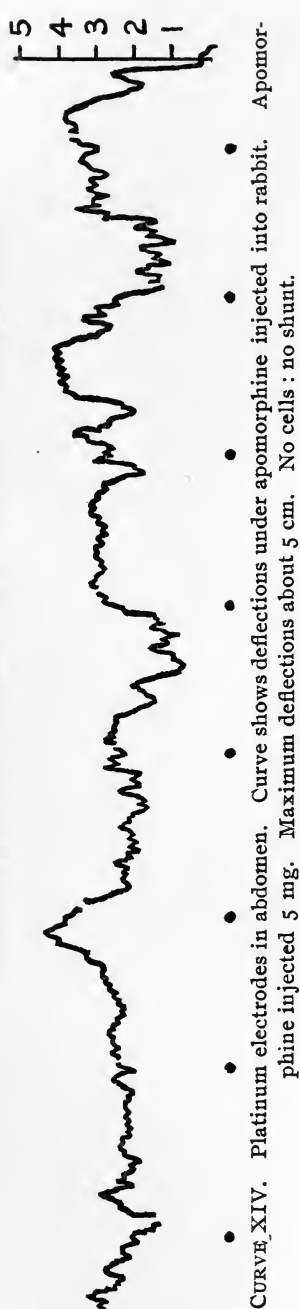
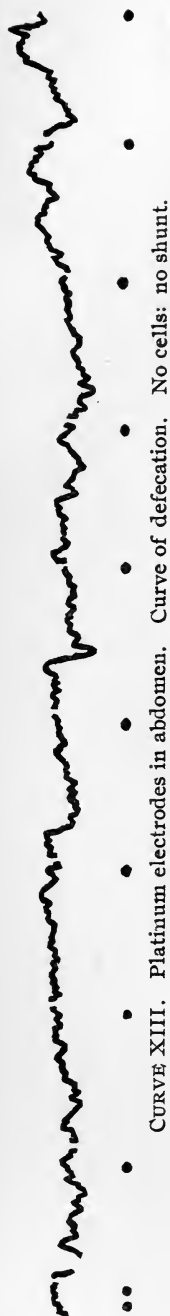
CURVE XII. Platinum electrodes in abdomen of rabbit. First part of curve normal. Then chloroform given. Marked deflections during struggle. Rest of curve shows no deflections. Rabbit under chloroform anaesthesia. Maximum deflection is about 5 cm. No cells : no shunt.

The foregoing (Curves XI., XII.) are the photographic records taken of the rabbit under the influence of chloroform and ether with or without stimulation.

## VII.

An examination of the photographic records under anæsthesia discloses the facts of large galvanometric perturbations when the anæsthetic is administered and again when the animal is passing from under the influence of the drug. Stimulations produce more or less marked deflections during the period preceding and following the state of deep narcosis. We notice one important circumstance and that is the fact, that such marked galvanometric deflections are uniformly accompanied by movements and struggles on the part of the animal. When the motor activity diminishes the galvanometric deflections decrease correspondingly and when the animal is quiet the galvanic perturbations completely disappear. The same relation is also observed in the case of the various drugs inducing peristalsis. Peristalsis accompanied by motor activity such as struggles, twitchings, shiverings, convulsions and generally by muscular contractions produce galvanometric deflections which seem to be proportionate to the extent of the observed muscular activity. Where motor activity is absent, although the action of the drug continues with its consequent peristalsis no galvanometric changes can be detected. Thus in the case of defecation which is accompanied by large contractions of the intestinal tract and general condition of straining there are large deflections, while during the intermediate periods of peristalsis, when the animal is quiet no deflections are present. This also holds true even of such cathartic drugs as aloin and croton oil. The curve of apomorphine is especially interesting from this standpoint. The injection of apomorphine into the rabbit does not produce vomiting, but causes continuous shivering and twitchings of almost all the muscles. The result is a corresponding ceaseless fluctuation of the galvanometric deflections. No less instructive is the injection of strychnine which gives rise to twitchings and convulsions with corresponding deflections of the mirror-galvanometer well brought out in the following photographic record:





The same relation holds true even in the case of the galvanometric deflections due to various stimulations. Where the stimulation is accompanied with motor reaction there the deflection is manifest, where such reaction is absent the galvanic deflection does not appear. All those facts point to the conclusion that the concomitant motor activity plays an important and possibly a predominant rôle in the causation of the galvanic phenomenon.

This agrees with the work of Sidis and Kalmus who have observed in their experiments that coughing, laughing, sitting, rising, bending arms and muscular activity in general give rise to marked galvanometric deflections. "From these experiments," they say, "it seems that muscular activity of those parts of the body actually forming the circuit bring about galvanometric deflections, while activity of the more remote parts are ineffective."<sup>1</sup> We certainly must take issue with Jung and Peterson in their claim that the galvanometric deflection due to coughing is 'psychic, that is, emotional.' The galvanometric deflection in coughing as well as in like physiological activities is entirely of muscular origin which may or may not be accompanied by an emotion.

That the obtained galvanometric deflection during stimulation and consequent contraction of muscles in the circuit is not the effect of movement of the electrodes inserted in the tissues of the animals can be demonstrated by the experiment of moving the electrodes to which are attached insulated rubber bands. Such movements of electrodes, but with no muscular contraction, give no galvanometric deflections. This is to be seen from the following photographic records :



CURVE XVI. No cells, no shunt. Platinum electrodes in legs of rabbit. Rubber bands attached to electrodes for insulation from touch of hands. Pulling hands and moving violently electrodes produced no deflections. When however a stimulation such as prick is given, the rabbit contracts the legs and a galvanometric deflection of 33 mm. is obtained.

<sup>1</sup> *Op. cit.*

## VIII.

If such relation between motor activity and the galvanic phenomenon exists, it should be demonstrated, after all other possible factors are rigidly excluded, by some crucial experiments.

The first crucial experiment that naturally suggests itself is to restrict the muscular activity of the animal and see what happens to the galvanic deflections, when the animal is stimulated by pinches, pricks, sharp snaps and various other painful agencies. If muscular contraction is concerned in the causation of the galvanic phenomenon, we should find that with their diminution and total suppression the galvanic phenomenon should be correspondingly decreased and even totally abolished. With this end in view we performed the following experiment :

The hind legs of the rabbit were firmly bound so that they could not move. The circuit was closed with the platinum electrodes inserted well into the muscles of the motionless thighs. Under such conditions no stimulations however painful could call forth galvanometric deflections. In other words, with the suppression of muscular action the galvanic reaction disappears. This is clearly demonstrated by the Curve XVIa.

With the platinum electrodes in the same position one of the legs was let free to move. When the rabbit was now stimulated the leg, of course, contracted and the galvanic deflections were evident in response to each stimulation. In other words, with the reinstatement of muscular action the galvanic phenomenon once more reappeared as demonstrated by the Curve XVII.

This experiment is crucial, inasmuch as it also excludes all other possible factors, such as secretion, whether of skin or of other glands ; it excludes circulation, whether of lymphatics or of blood-vessels and excludes also the action of the sympathetic and of the central nervous system. For if the galvanic phenomenon is due to any, or all of those physiological processes, the galvanic phenomenon should be present under the influence of stimulation, since those physiological processes are not arrested with the restriction of the movements of the limb.

CURVE XVIa. Platinum electrodes into hind legs of rabbit. Legs immobilized. Rabbit stimulated every minute. No deflections. No cells: no shunt.



CURVE XVII. Platinum electrodes into hind legs of rabbit. One leg free. Rabbit stimulated at intervals of one minute. Deflections with each motor reaction to painful stimulation. Maximum deflections about 7 cm. No cells: no shunt.

Of course, the skin-effects have practically been excluded by the whole course of our experiments, inasmuch as we worked exclusively with subcutaneous electrodes and still obtaining the galvanic deflections in response to various stimulations.

That the skin effects or secretion-currents<sup>1</sup> have nothing to do with the galvanic phenomenon can be further shown by the experiment that when the electrodes are inserted into the skin only, the deflections are made to disappear with the immobilization of the limbs as shown by the Curve XVIII.

In experimenting on the cat similar results are obtained. When the cat is immobilized no sensory stimulations, such as pricking or pinching, can possibly produce any galvanometric deflection. When however the movements of the animal are made somewhat freer so as to make possible muscular contractions the galvanic perturbations under the influence of sensory stimulations become manifest.

Experiments performed on the frog exclude skin resistance and glandular skin secretion as possible factors in the causation of the galvanic phenomenon.

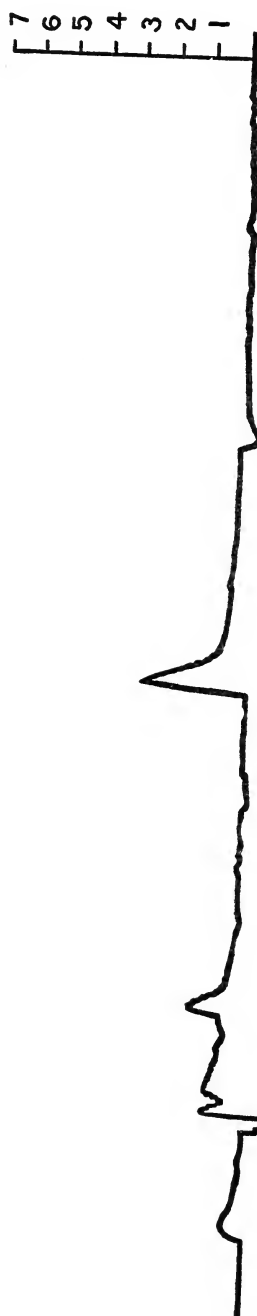
<sup>1</sup> 'Secretion-currents' are usually ascribed to the physiological activity of the secretory glands. Our experiments, though not final, seem to point to the fact that in 'secretion-currents' we do not deal at all with physiological activities, but with purely chemical processes going on in the end-products of secretion. The chemical processes of the secreted end-products give rise to electrical currents which are regarded as secretion-currents representing the physiological activity of the glands. Such currents however may have little or nothing to do with glandular physiological activity and may be nothing but an *artefact* due to chemical processes going on in the decomposition of the secreted products.

If one of the platinum electrodes is put into the inner surface of the armpit rich in glands and the other platinum electrode is put on the shoulder, there is a marked galvanometric deflection. If now we take cotton and saturate it with the secretions from the armpit and then let the cotton soak in a small beaker filled with distilled water and immerse one of the platinum electrodes in the beaker and the other platinum electrode in another beaker with pure distilled water, *the galvanometric deflection is found to be of the same order of magnitude and in the same direction as in the experiment on the armpit and the shoulder*. The same result is obtained when the platinum electrode is applied by pressure directly to the saturated cotton. The 'secretion-currents' here are evidently not physiological.

The subject of secretion is highly complicated and cannot be dismissed with a couple of experiments, however suggestive. We shall take up the matter in a separate study on secretion as an accompaniment of psycho-physiological activity.



CURVE XVIII. Platinum electrodes through skin of hind legs. Rabbit immobilized. Painful stimulation given at intervals of one minute; no deflections. No cells: no shunt.



CURVE XVIIIa. Platinum electrodes in legs of strapped cat. Where the animal can react with muscular contractions to stimulations there is a deflection which diminishes and disappears with greater and even complete limitation of muscular movement.

If the frog is put on the animal board, the platinum electrodes put into the muscles and the animal well bound and stretched out on the board so as to arrest muscular activity, the galvanic deflections due to stimulations diminish with the restriction of muscular activity and disappear with the complete arrest of muscular reaction to external stimulation. The galvanic phenomenon remains absent when the platinum electrodes are wound around the freely secreting skin of the frog, or on the inside of the skin layer, or one electrode is put on the outside and one on the inside of the skin. In all such cases, provided the muscular activity of the frog is arrested, the galvanic phenomenon is absent.

If the frog is curarized, thus abolishing the action of the muscles, but not affecting sensitivity, the platinum electrodes inserted into the muscles call forth no galvanic deflection. If the electrodes are now put into the skin or on the inside and outside of the skin layers, no sensory stimulation, however violent, can call forth the galvanic phenomenon.

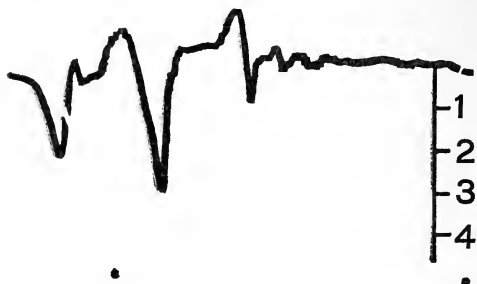
That the glandular secretion has nothing whatever to do with the galvanic phenomenon can be further demonstrated by the following experiment:

The skin of the frog is easily removed from both legs leaving exposed the muscles of the legs into which the platinum electrodes are inserted. When the galvanometer is at zero and remains stationary, the animal, with legs free, is stimulated by sharp pricks or pinches, with each stimulation and concomitant muscular reaction there is a marked galvanometric deflection amounting, in some cases, to more than 20 millimeters. Under such conditions the following characteristic curve is obtained (Curve XVIIIb):

The brain, the spinal cord, the sympathetic nervous system as well as the action of other internal organs, such as liver and spleen, have likewise been *directly* eliminated by us. We plunged our platinum hypodermic electrodes into the tissues of those various organs and found that when muscular contractions were not present the galvanic phenomenon was invariably absent.

Similarly circulation can be directly excluded. Already Sidis and Kalmus excluded circulation as the cause of the

galvanic phenomenon by the use of Esmarch bandages. In the case of animals, such as the rabbit or the frog, it is possible to exclude circulation by ligation of the arteries supplying the



CURVE XVIIIb. Platinum electrodes inserted in legs of frogs. Legs stripped of skin. First part of curve normal. Second part shows marked galvanometric deflections under the influence of stimulations (pricks, pinches) concomitant with muscular reactions.

limbs. Under such conditions the galvanic phenomenon still persists showing that blood circulation is not among the causes of the galvanic phenomenon. The following (Curve XIX.) is a photographic record of such experiments :

That the galvanic reaction is entirely muscular can be still further demonstrated by the following experiment :

The sciatic nerves were cut and platinum electrodes inserted into the muscles of the legs. Under such conditions the galvanic phenomenon was absent. No stimulations, however intense and painful given in different parts of the body, could call forth the galvanic phenomenon as shown by the following photographic record : (Curve XX.)

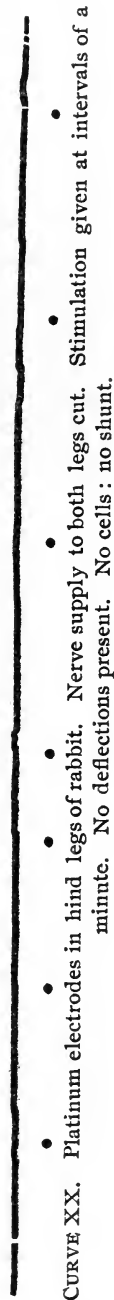
Similar experiments were also performed on frogs and with the same results. With the platinum electrodes in the gastrocnemius of each leg the galvanic phenomenon invariably disappeared when the sciatic nerves were cut. The following curve (Curve XXI.) is a photographic record of the experiment :

The experiment of section of the motor nerves of the legs is also a crucial one, inasmuch as the galvanic phenomenon disappears on the paralysis of muscular activity, although all other conditions, skin secretions, circulation and sensory nerve processes remain unchanged. Moreover, it may be added that





CURVE XIX. Platinum electrodes in left hind leg of rabbit. Left femoral artery ligated. Left leg free. Rabbit stimulated at short intervals of 20 seconds. Marked deflections with each stimulation. Maximum deflection about 3 cm. No cells: no shunt.



CURVE XX. Platinum electrodes in hind legs of rabbit. Nerve supply to both legs cut. Stimulation given at intervals of a minute. No deflections present. No cells: no shunt.

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CURVE XXI. Platinum electrodes wound around gastrocnemius muscle of frog. Sciatic nerve cut. Leg free. Frog stimulated at intervals of 20 seconds. No deflections to stimulations. No cells : no shunt.

the galvanic deflections can be reinstated even under conditions of paralysis of motility by passive contraction of the muscles of the leg, as demonstrated by the following photographic record : (Curve XXII.)

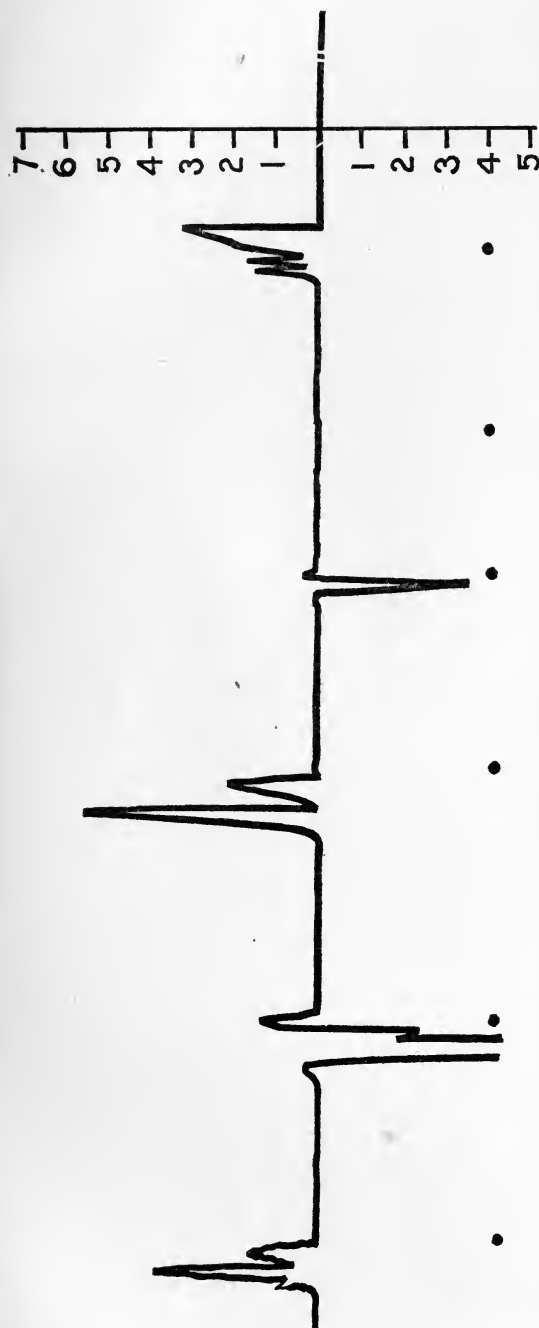
We can now explain the large galvanometric perturbations obtained in the case when the hypodermic electrodes are inserted into the abdominal wall. The animal in all of our experiments was tied on a board so that the extremities were naturally more limited in their movements than the abdomen, which remained free to react to painful stimulations.

We are also in a position to account for the significant fact, present in all of our experiments, namely, that struggles, twitchings and convulsions are followed by large galvanometric deflections. *For our work proves conclusively that the galvanic reflex is a muscular phenomenon. The galvanometric deflections are due to electromotive forces liberated by muscular activity under the influence of affective and emotional states.*

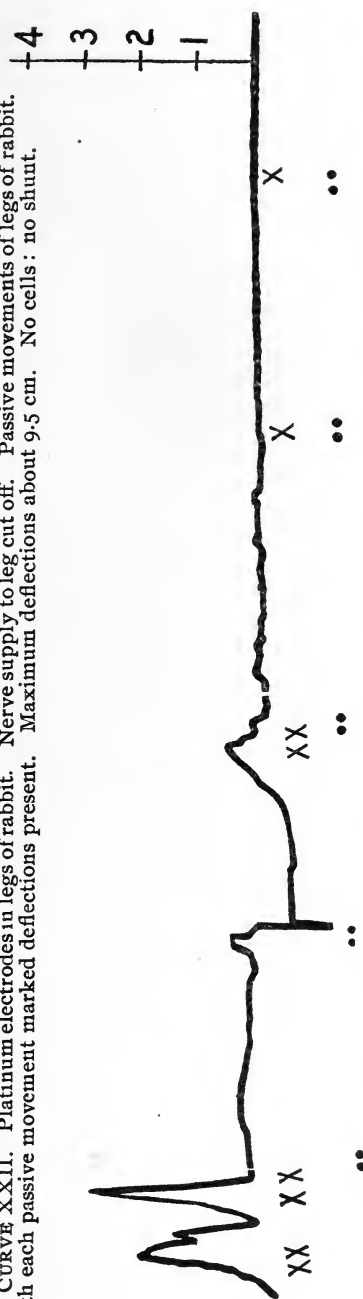
Another crucial experiment is that of injection of curare. It is well known that curare only affects the striped or voluntary muscles leaving all other functions unimpaired. Now when the frog or the rabbit is injected with a dose 2 c.c. of 1 per cent. solution of curare and kept alive by artificial respiration the galvanic phenomenon completely disappears. The paralysis of muscular activity causes this disappearance of the galvanic phenomenon. The following photographic record shows the results of the experiments under the influence of curare : Curve XXIII.

Marked rhythmical deflections are obtained from muscular contraction of heart as shown by Curve XXIV.

We can now understand the reason of the apparent paradox so puzzling to Jung and Peterson when they say "there are features presented which are as yet quite inexplicable, as for instance, the gradual diminution of the current in long experiments to almost complete extinction, when our ordinary experience teaches

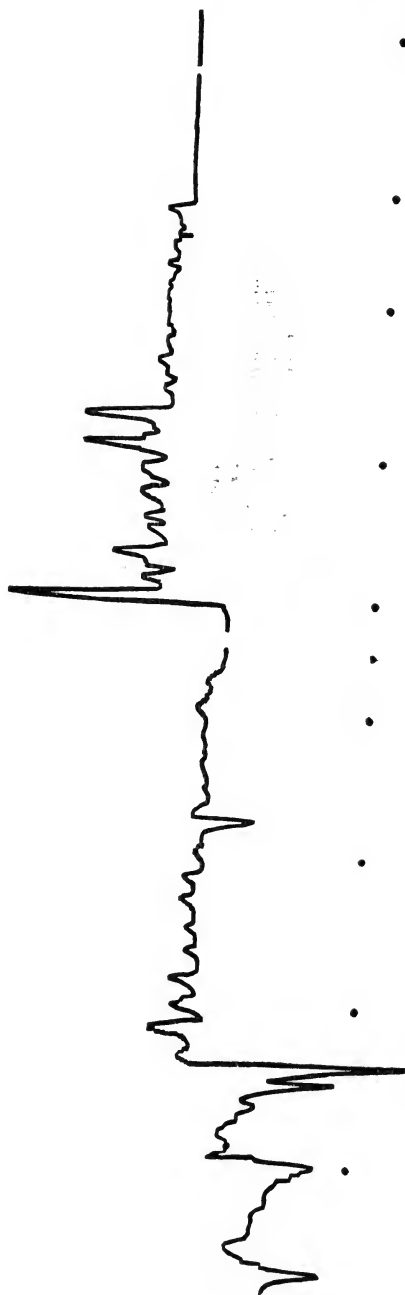


CURVE XXII. Platinum electrodes in legs of rabbit. Nerve supply to leg cut off. Passive movements of legs of rabbit. With each passive movement marked deflections present. Maximum deflections about 9.5 cm. No cells: no shunt.

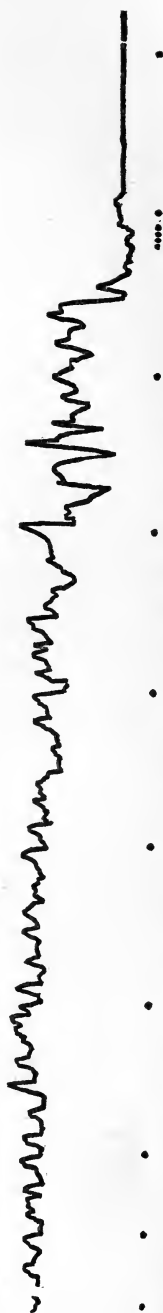


CURVE XXIII. Platinum electrodes in abdomen of rabbit. Rabbit given 2 c.c. of 1 per cent. solution of curare. Stimulation gave no deflections. Deflections are obtained by slight passive movements of legs and by tapping or rubbing abdominal walls, deflections amount to 35 mm. No cells: no shunt. (x) shows stimulation. (xx) shows passive movement of legs.

CURVE XXIV. Platinum electrodes wound around heart of rabbit. Rabbit injected with 4 c.c. of 1 per cent. solution of physostigmine. Deflections are synchronously with the contractions of the cardiac muscle. No cells: no shunt.

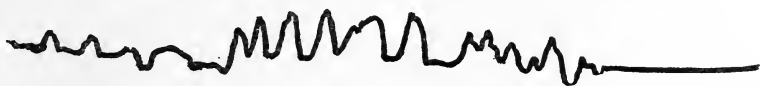


CURVE XXV. Platinum electrodes in hind legs of rabbit. No cells and no shunt. Legs are free. After a normal is taken a series of stimulation of slight touches every 10 seconds is given to the leg of the rabbit. The galvanometric reaction reaches its maximum of 6.5 cm. with violent muscular reaction to subside for a period of 4 minutes, after which the galvanometric reaction once more reaches its maximum and so on.



CURVE XXVI. Platinum electrodes in hind legs of rabbit. Right leg free. A series of hard pricks given to rabbit at regular intervals of 10 seconds. The galvanometric reaction gradually diminishes falling from 30 mm. to about 3 mm. No cells: no shunt.

that resistance should be much reduced and the passing current larger and 'stronger.' The reason why Jung and Peterson find the fact of 'the gradual diminution of the current' so 'inexplicable' is because they have totally misconceived the nature and cause of the galvanic phenomenon. In the first place, we do not deal here at all with resistance, but with an electromotive force. In the second place, the electromotive force generated is muscular in origin. This makes 'the gradual diminution of the current in long experiments to almost complete extinction' an absolute necessity. For it is clear that an electromotive force cannot possibly become stronger and larger 'with continuous use.' That would be against all the laws of physics. With continuous use the muscles become exhausted and with the repetition of the same stimulus a lesser impression is made on the sensory nervous system calling forth a smaller and smaller muscular reaction with its accompanied diminution of electromotive force and consequent decrease of galvanometric deflection. This is demonstrated by Curves XXV, XXVI, XXVII.



CURVE XXVII. No cell, no shunt. Platinum electrodes in legs of rabbit. Legs free. Passive movement of legs every 10 seconds. Curve shows diminution from 20 mm. to 5 mm.

*We may say then that all our experiments prove incontestably that the galvanic phenomenon is due to an electromotive force which is muscular in origin.*

### VIII.

In conclusion we may make the following summary of our results :

1. Galvanometric deflections are brought about by psychophysiological processes (but not by purely ideational processes) under the influence of various stimulations.
2. These galvanic deflections termed by us 'galvanic reactions' are not due to variations of resistance, whether of skin or of body.
3. The galvanic reaction is the *result of variation of elec-*

*tromotive forces produced* by the psycho-physiological processes set into activity by the agency of external or internal stimulations.

4. The causation of the galvanic reactions cannot be referred to circulation, nor can it be referred to secretory currents, whether of skin-glands or of other glandular organs.

5. The central nervous system and the sympathetic nervous system are alike excluded as factors concerned in the manifestation of the galvanic reaction.

6. *The galvanic reaction is entirely a muscular phenomenon* due to contraction, stretching, straining of the muscular fibers under the influence of various agencies, be they psychic, sensory, physiological, chemical, thermal, electrical or mechanical.

7. The galvanic reaction is chiefly brought about by the muscles within the circuit.

8. Prolonged active peristalsis gives rise to galvanic deflections which are due to the contraction of the muscles involved in the process of peristalsis.

9. The galvanic reaction diminishes and even completely disappears with the repetition of the same kind of stimulation.

10. This fall or complete disappearance of the galvanic reaction with the repetition of stimulation is usually due to a decrease of sensitivity in regard to the same repeated stimulation.

11. The fall however of the galvanic reaction may also be brought about by the action of a prolonged stimulation resulting in a gradual fatigue of the muscles in the circuit.

12. The heart-beat, like the contractions of any of the other muscles, gives rise to galvanic deflections.<sup>1</sup>

We are glad to thank Professor Franz Pfaff, of the Pharmacological Department of Harvard Medical School, for the many opportunities and courtesies shown us in the carrying out of this experimental work.

<sup>1</sup> The clinical aspect of the galvanic reaction will be considered by us in a separate study.

## PERSONAL DIFFERENCES IN SUGGESTIBILITY.

BY PROFESSOR WALTER D. SCOTT,

*Northwestern University.*

Discussions upon suggestion and suggestibility ordinarily seem much influenced by the older concepts taken from 'faculty' psychology. In that former time observation, for instance, was assumed to be an unanalyzable and elementary faculty. One person might possess a good power of observation and another a poor one. Individuals of a group could be given some single test and thus classified and given rank as to their power of observation in general. To-day we speak of suggestibility in an analogous fashion. This manner of thought is not confined at all to the laity but finds explicit expression in most of the recent technical discussions of the subject. The statements are made without reservation that children are more suggestible than adults; that girls are more suggestible than boys; that some nations are more suggestible than others; that neurasthenics, psychasthenics and hystericals are peculiarly suggestible; and finally that any particular group of individuals could be definitely ranked according to the degree of their suggestibility.

In an attempt to determine to what extent suggestion is a general faculty and therefore to what extent individuals could be ranked as to the degree of their suggestibility, a single group of individuals has been tested by various methods. The results secured by the different methods were then correlated by the well-known Pearson's formulæ for the coefficient of correlation.

For the most part the methods employed were taken directly from the literature describing methods for testing suggestibility and need no further comment. Two of the methods or devices, however, were, at least in part, new and seem especially well adapted for testing suggestibility.

The first method was one for controlling by suggestion the

sequence of colors in the visual after-image secured from fixating white light.

The subjects were college students who were just beginning their first course in experimental psychology. Many of them were at the time experimenting upon negative after-images for colored papers.

For the experiment upon suggestion each student was taken singly into the room specially prepared to increase the suggestibility for the particular experiment. A mounted spectral chart was demonstrated and the spectral order of the colors committed by the student. The Bradley disk, composed of six sectors, each of a single spectral color, arranged in order, was placed on the color wheel and revolved till a pure gray was secured. A prism was used to separate the rays of white light into the several spectral colors. The student was then instructed that just as the prism analyzed the white light into the spectral colors so an after-image from white light would contain the spectral colors in sequence. He was told that the experimenter was securing data as to the exact time various subjects required for observing in sequence the various spectral colors as they develop in the after-image.

The experimenter sat before a kymograph with a time marker which marked seconds on the smoked paper. The light necessary for his work was not sufficient to add greatly to the general illumination of the otherwise dark room.

The white light for fixation was secured by means of an opening in a screen twelve centimeters square. The subject sat three meters from the screen and by raising his eyes about thirty degrees he looked through the opening and directly into the white skylight.

The subject fixated this white skylight for twenty seconds, then closed his eyes and had them further screened by several thicknesses of black velvet.

At the conclusion of the fixation the experimenter began at once to increase the previous suggestions by such questions as the following: Let me know as soon as the red appears. Report the red as soon as it comes. Is it red yet? Now is it red?, etc. These questions were continued for 20 seconds or until red was



reported, when the same questions were asked for orange, yellow, and so on through the spectral order.

The subject was of course instructed to report whatever colors he saw and to report them the first moment possible. The experimenter recorded on the revolving drum the color reported and the time of the report.

After the subject's eyes had fully recovered from the effect of the after-image, a second and a third trial was made.

In some instances the after-image lasted several minutes but occasionally it would fade before 30 seconds succeeding the first appearance of any color. For this reason we considered only the colors reported for the 20 seconds succeeding the first report of the presence of any color in the after-image.

With conditions such as those under which we worked the normal sequence of the after-image is first a blue, then a green, then red and then finally a blue. The colors are very brilliant and beautiful. The red ordinarily does not appear during the first 20 seconds. Therefore that red which is reported during the first 20 seconds is recorded as due to suggestion. Apart from suggestion an orange would certainly not succeed a red during the first 20 seconds. Any advance in the spectral order beyond red during this first 20 seconds is unmistakably due to suggestion.<sup>1</sup>

Adding together the first 20 seconds in each of the three trials we get a total of 60 seconds in which the effect of suggestion was being measured. One subject reported as follows: First trial — red, orange, yellow, green, blue and violet; second trial — red, orange, yellow and green; third trial — red, orange, yellow, green, blue and violet. This is a total of 16 suggested colors in 60 seconds. The colors reported were not indistinct but vivid and beautiful. In no way did he suspect that the colors were due to suggestion. As a control at a later time we changed the conditions but not in any essential particular except that we instructed the subject that with the new conditions the spectral order would not be secured. In three trials he reported

<sup>1</sup> No adequate data are available indicating the manner in which the sequence of the colors of the after-images for white light is varied by changes in illumination, time of fixation, etc. The writer contemplates the securing of such data.

the normal sequence, with certain variations as is common to this experiment. We estimated the results of his last trials, namely, those without suggestion, upon the same basis as was used in securing his previous record of 16, and his new record was 1. In one of the three trials the red appeared before the limit of 20 seconds. In no case, however, in the control experiments, did an orange succeed a red.

Of the twenty students tested 3 reported no color due to suggestion and the others ranged from 1 to 16 as is indicated in the following table.

Subject.	No. of Suggested Colors Reported.	Rank in Group of 20.	Times Suggested Heat was Reported.	Rank in Group of 20.
<i>A</i>	16	1	10	5
<i>B</i>	10	2	10	5
<i>C</i>	7	4	9	12
<i>D</i>	7	4	8	15
<i>E</i>	7	4	10	5
<i>F</i>	6	7	9	12
<i>G</i>	6	7	4	18
<i>H</i>	6	7	5	17
<i>I</i>	5	9	10	5
<i>J</i>	3	11½	3	20
<i>K</i>	3	11½	10	5
<i>L</i>	3	11½	10	5
<i>M</i>	3	11½	4	19
<i>N</i>	2	15	9	12
<i>O</i>	2	15	10	5
<i>P</i>	2	15	10	5
<i>Q</i>	1	17	10	5
<i>R</i>	0	19	7	16
<i>S</i>	0	19	9	12
<i>T</i>	0	19	9	12

The same 20 students acted as subjects in an experiment upon the production by suggestion of illusions of heat.

An electric current of 110 volts, direct current, was sent through a bank of lamps and then through a naked wire of high resistance coiled about a lead pencil. The strength of the current passing through the coil was so adjusted that a subject could detect the change of temperature in from 5 to 10 seconds. The lamps were placed on the center of an ordinary sized table and the naked coil was supported a few inches above the table and near its edge. A subject could then sit at the table facing the lamp and grasping the naked coil with two fingers and a thumb. In this position the light of the lamps in the otherwise dimly

lighted room flashed brightly in his face. The heat from the lamps could be slightly felt on the face but was shielded from the coil and the hand grasping it.

The subject was instructed that the experiment was to secure the lower threshold for temperature and that 20 readings would be necessary. He was shown how the current which heated the lamp passed through the naked coil heating it also. He was shown how the wire would gradually increase in temperature till it became appreciably heated. He was told that he was to hold the wire in a particular way and to report the fact as soon as the temperature of the wire appreciably changed.

The procedure of the experiment was well stereotyped. After a warning the signal was given. The stop watch was then started, the electric switch closed, the subject grasped the naked coil and the lamps were lighted. The experimenter then kept one hand on the switch and the other on the stop watch in an attitude of strained attention. At the signal, 'now' from the subject the watch was stopped, the switch opened, the subject removed his hand from the coil and the lamps went out. This was repeated for 10 trials. From the eleventh to the twentieth trial nothing was changed except that the experimenter touched a concealed switch with his knee which shunted the current off completely from the naked coil without in any way reducing the amount of current passing through the lamps. Even when no heat was thus generated in the coil the subject might continue to report the presence of the heat just as regularly as during the first 10 trials.

The experimenter made a rough estimate of the average time for the first 10 trials, and then during the latter 10 trials if the subject failed to report the presence of heat within a time 5 seconds in excess of his previous average, the experimenter removed his knee from the concealed switch and sent the current through the coil. For instance, if during the first 10 trials the subject had reported heat on the average at 7 seconds, then if during one of the latter trials he had not reported heat by the twelfth second, the heat would be sent through the coil, so that ultimately the heat would actually be felt and no suspicion aroused.

As would naturally be expected, some of the subjects re-

ported heat as regularly during the trials from number eleven to twenty as from number one to ten. Some even gradually shortened the time for the second half of the series. Others failed to report heat except when it was actually being generated by the current. Some reported heat occasionally when none was present.

This experiment with heat has many points of similarity with the previous experiment with after-images. Both are concerned with sense-suggestion. In each an attempt is made to change one sensation into another by means of suggestions. In the first an attempt is made to change the normal blue of the after-image to a suggested red and then to other suggested colors. In the latter experiment an attempt is made to change by suggestion a perception or sensation of touch into one of heat. In each the experimenter employs his personality in securing expectant attention through instruction as to what is to be expected. The subjects were experimented upon singly. The time marker in the first and the stop watch in the second set of experiments prompts to hasty reports. Indirect factors enter into each. The sight of the analysis of white light into the spectral colors in the one and the warmth reaching the face from the lamps in the other are the most significant indirect factors in rendering the suggestions effective. In both experiments the suggested results are fully expected. This was shown by the introspection of the subjects which was secured in each instance. No suspicion was indicated in any of the reported introspections.

The conditions surrounding the experimentation and the psychological factors experimented upon seemed so similar in the two experiments that a fair degree of correlation was anticipated. When, however, the coefficient of correlation was secured from the two sets of data presented in the table on page 150 it proved to be insignificant.

The most natural interpretation of this result is that the suggestibility tested by the one experiment was different from that tested by the other.

Among the psychological factors which differ in the two sets of experiments a few are apparent. Suggestibility in the first case may be dependent upon strength of visual imagery, and

in the second case upon strength of imagery for temperature. If such is the case, then to secure tests upon suggestibility and not upon the relative strength of imagery the experiments would have to be so changed that the demand would be made upon imagery of the same sensorial type, and only those subjects might be included in the group who had relatively equal degrees of strength of this type of imagery.

But perhaps the factor most effective in reducing the coefficient of correlation secured for these two tests lies in the source of the expectancy. In the experiments upon after-images the expectancy was based mainly on the word of the experimenter. The working of the prism and of the color wheel may have been, in some instances at least, negligible factors. In the experiment with heat the word of the experimenter may have been negligible while the experience secured in the first 10 trials with heat may have been, in some instances at least, the sufficient ground for the expectancy. The experiment with after-images may then have been a test of hetero-suggestibility, while the experiment with heat may have been an experiment upon auto-suggestibility. To eliminate this psychological difference in the form of suggestibility being tested it would be necessary to increase the hetero-suggestion in the one or to reduce it in the other. The auto-suggestion in the first could be increased by so modifying the experiment that the subject would have his expectancy aroused by his previous experience. To secure this the subject could be informed that the sequence of the colors in the after-image is blue, green, red and blue. He could then be led to observe the after-images on ten bright days in which the actual order of sequence of the colors of the after-image would be according to his instructions. On his eleventh trial he would expect the same order even though the sky were less bright and therefore another order would normally result. If however he was instructed that the order, blue, green, red and blue was universally secured, and if in addition he had secured that order in ten trials his expectancy would be at least similar to that awakened for the heated wire upon the eleventh trial. In each case the words of the experimenter are corroborated by ten or more instances from the past experience of the subject.

There may be still other psychological factors in addition to differences in imagery and differences in susceptibility to hetero-suggestions and to auto-suggestions which had a part in lowering the coefficient of correlation.

At all events the inference from a study of these two experiments (and others not here described) is that degrees of suggestibility as determined from one test cannot be inferred as holding for suggestibility in general. Before individual *A* can be said to be more suggestible than individual *B* they must have been subjected to many and diverse forms of tests. Otherwise different degrees of suggestibility should be affirmed as present only for the particular form or forms as tested.

Suggestion, like observation, is a general term embracing many psychological processes. We have ceased to speak of people as possessing great powers of observation and instead we specify the particulars in which the power of observation has been shown to be especially good. In studying the personal differences in suggestibility we must adopt a form of expression analogous to the newer forms used in discussing observation. Instead of speaking of high and low degrees of suggestibility in general we are forced to specify the particular in which the degree of suggestibility has been observed.

## ANNOUNCEMENT.

THE publishers regret to announce the resignation of Professor J. Mark Baldwin as editor of these publications after sixteen years' activity. The *PSYCHOLOGICAL REVIEW* was founded by Professors Baldwin and Cattell in 1894. It was successful from the start, and has become the center of several publications covering different types of contributions to psychology. The Review Publications will hereafter be conducted by the present editors of the *REVIEW*, *BULLETIN*, and *MONOGRAPHS*.

Contributions, books, and editorial correspondence may be addressed to any one of the responsible editors.

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# THE PSYCHOLOGICAL REVIEW.

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## THE COMPLICATION EXPERIMENT AND RELATED PHENOMENA.<sup>1</sup>

KNIGHT DUNLAP,

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### I. EXPERIMENT WITH MYSELF AS SUBJECT.

While engaged as a subject in Dr. Burrow's experiments on complications and purely visual combinations<sup>2</sup> I was impressed with one fact which seemed to me significant. The moving pointer or index seemed fairly definite at the moment when I succeeded in obtaining a judgment which was subjectively satisfactory; the pointer was not absolutely sharp and distinct, it is true, but it was much more definite than I would expect, at the speeds of rotation employed, if the eye were really stationary.

This observation suggested that my fixation was not maintained at the moment of the apparent alignment, *i. e.*, of the coincidence of the pointer with the radius of the fixed mark, but followed the pointer for a very small distance. This possibility was suggested by consideration of the well-known phenomenon which appears when the eyes are moved across a disc of black and white sectors revolving fast enough to blur; frequently one of the sectors will flash out strongly, seeming to stand still momentarily. This *flashing sector* phenomenon is explained by Dodge<sup>3</sup> as due to a brief coincidence of the rate of movement of the sector with the rate of movement of the eye, the time of coincidence being long enough to give a sharp retinal image of the sector. Evidently, if the fixation, in Burrow's experiment, should deviate in the direction of movement

<sup>1</sup>From the Johns Hopkins Psychological Laboratory.

<sup>2</sup>Burrow, N. T., 'The Temporal Position of a Momentary Impression, etc.,' 1909, *PSYCHOL. REV.*, *PSYCHOL. MONOGRAPHS*, Vol. XI., No. 4.

<sup>3</sup>Dodge, R., 'The Participation of the Eye Movements in the Visual Perception of Motion,' 1904, *PSYCHOL. REV.*, XI., 1-14.

of the pointer even for an exceedingly short distance, a passable image of the pointer might be obtained.

The observation I have just described did not thrust itself upon me until towards the close of Burrow's work, for which reason largely, it led to no experimental issue in his hands, although communicated to him. I therefore decided to investigate the phenomenon myself, and did so in the period included between the middle of May and the middle of July, 1909. No subjects were available during that portion of the year so that I was obliged to perform the experiments on myself, as described below.

I used Burrow's apparatus<sup>1</sup> (with the modifications I shall describe), it being eminently suited to the work. My first endeavor was to find some means for steadying the fixation, and after casting about among several devices I finally adopted a piece of ordinary white mosquito-netting stretched over the half-dial of the apparatus and the half of the disc adjacent to the dial, as close to the surfaces of disc and dial as could be without rubbing on the disc. By this means I was provided with excellent fixation objects where the threads of the netting crossed the black fixed mark or 'goal.' The netting had meshes about 2.5 mm. square.

My first observation, on attempting to make judgments with the apparatus at one of the standard rates used by Burrow (1.25 sec. per rotation) was that *the meshes of the netting blurred* as the pointer went by the goal, although there was no other sign of disturbance of fixation. Evidently, there was either movement of the eye, or change of accommodation. My first efforts were directed to the maintenance of the sharp image of the threads, and after several hours' practice — on several days — I attained this fairly well; that is, I could keep the meshes reasonably distinct in most of the tests. But *the pointer now blurred* as it passed the fixed mark. This preliminary practice, and the succeeding work, were very trying to my eyes: so much so that very little experimentation, sometimes only a half-hour's work, could be done on one day, and my eyes were in an irritated condition for several weeks after the experiment concluded.

<sup>1</sup> Burrow, *op. cit.*, pp. 23-27.

In place of the discrete stimuli (flash of Geissler tube or stroke of sound-hammer) employed by Burrow, I substituted the click of a telephone receiver held in a clamp at about six inches from my right ear, as I stood in a position to make judgments. The telephone receiver was actuated by the 110 v.d.c. lighting current, with one carbon filament lamp in series, and a low resistance rheostat (about 50 ohms) in parallel with it. Contact was made through the mercury cup on the time machine, which was so adjusted that the travelling contact-point barely brushed the very crest of the convex mercury surface. In this way there was no appreciable interval between make and break of contact, hence practically a single loud click in the receiver; and there was almost no spark at the mercury cup.

My first set of results consisted of 100 determinations; for each of the rates .75, 1.25 and 1.75 secs. per rotation. Ten 'A' and 10 'B' determinations at one rate constituted the work for one day. My method of experimentation was as follows: Having moved to one side the lever which controlled the relative position of the discrete stimulus (*L* in Burrow's diagram and description), and having the contact-arm over the mercury cup, and the pointer approximately opposite the lever, I seated myself and shifted the pointer an unnoted distance towards the fixed mark, and then started the motor. As, after I had shifted the position of the pointer, I did not see the disc until it had gained its normal speed of rotation, I did not know the *starting position* of the click, *i. e.*, the position of the pointer at which the click occurred as I began to make judgments in any given test, nor did I know the position of the finger-lever corresponding to actual simultaneity of click and alignment.

After completing the determination, *i. e.*, moving the lever until the alignment and click seemed simultaneous, I stopped the motor, brought the contact-point to exact position at the mercury cup, and read the position of the pointer. Then, moving the finger-lever to the other side ('A' if the first position was 'B' and *vice versa*), I shifted the pointer in the opposite direction from the former shift; *i. e.*, again *towards* the fixed mark; started the motor, and proceeded as before. In this way I carried through the set, without any bias except from my

knowledge of the results. As a matter of fact, I was unable to recall on one day what my determinations had been on the day before; even what their general trend had been.

The starting position did not seem to vary within as wide limits as those used by Burrow when I was subject ( $75^\circ$  to  $45^\circ$  from the fixed mark), ranging *perhaps*  $35^\circ$  to  $60^\circ$  from the fixed mark; but of course I have no data on the point.

TABLE I.

RATE 1.25 SECS. PER ROTATION.  $1^\circ = 3.47\sigma$ .

Series.	'A.'	m.v.	'B.'	m.v.	Average.
1	+3.7	4.10	-7.3	4.56	-1.80
2	-1.2	3.92	-2.7	6.66	-1.95
3	+0.1	3.92	+1.8	7.36	+0.95
4	+4.4	6.28	+1.2	9.56	+2.80
5	-1.4	13.92	-0.1	3.9	-0.75
Averages	+1.12		-1.42		-0.15
$\sigma$	+3.88		-4.92		-0.52

TABLE II.

RATE 1.75 SECS. PER ROTATION.  $1^\circ = 4.86\sigma$ .

Series.	'A.'	m.v.	'B.'	m.v.	Average.
6	+0.6	2.2	+0.8	2.04	+0.7
7	+1.0	1.8	-1.0	3.60	0.
8	-1.9	5.12	-0.4	2.60	-1.15
9	+0.4	4.4	+0.7	3.96	+0.55
10	+0.7	2.3	+0.2	2.56	+0.45
Averages	+0.16		+0.06		+0.11
$\sigma$	+0.77		+0.29		+0.53

TABLE III.

RATE 0.75 SEC. PER ROTATION.  $1^\circ = 2.08\sigma$ .

Series.	'A.'	m.v.	'B.'	m.v.	Average.
11	-1.7	4.05	-4.8	5.84	-3.25
12	+2.7	9.20	+6.5	7.0	+4.6
13	+1.3	4.16	-3.5	7.12	-1.1
14	-2.6	9.00	-0.6	10.08	-1.6
15	-3.6	7.32	-1.6	5.2	-2.6
Averages	-0.78		-0.8		-0.79
$\sigma$	-1.62		-1.66		-1.64

The results of these experiments are given in Tables I., II. and III., in which the average displacement for 'A' and 'B,'

the m.v., and the average for 'A' and 'B' together are given for each day (each set of 20 determinations). The results are given in degrees, and the averages of the columns (of all the A's, of all the B's, and of all of both together) in *sigma* also.

The average total displacement for each rate is practically negligible; a trifle over one half *sigma* for the 1.75 rate, and the 1.25 rate, and a trifle over one *sigma* for the 0.75 rate. Compare this with my determination in Burrow's experiments (subject I., Table I., Sound: p. 31 of Burrow), which gave the following errors, expressed in *sigma*.

Rate.	'A.'	'B.'	Total.
0.75	— 23.3	— 22.9	— 23.1
1.25	— 46.0	— 51.2	— 48.6
1.75	— 36.2	— 45.1	— 40.6

The sophistication due to my current knowledge of my individual errors *may* have been consequential, although there is no indication thereof in the results: there is no progressive change either in kind or amount of error from series 1 to 5, 6 to 10, or 11 to 15. To exclude all knowledge of results it was necessary to conduct experiments in a different manner and this I proceeded to do, obtaining the results given in Table IV. by the technique described below. Removing the circular 'dial' with the single mark, I substituted the 'dial' with uniform scale of 5 degree intervals, I shifted the cardboard disc without observing the amount of the shift by giving it a jerk in one direction, a jerk in the opposite direction, and then a third jerk in the first direction. Then, standing, with my gaze turned to one side, I shifted the finger lever to some position without noting it, and started the motor. After regular rotation was established, I determined the position of the pointer at which the click seemed to occur. I then read off the position of this point on the dial: and then the position of the finger lever as given by the brass scale, and recorded both. Then, after shifting the finger lever some distance inattentively, without stopping the motor, I proceeded to judge the new point of entry of the click. In this way I proceeded until twenty determinations had been made, after which I stopped the machine, set the finger lever at zero on its scale, and wrote down the corresponding reading for the pointer

on the dial. The next day I shifted the cardboard disc again, and proceeded as before. After completing the set of experiments, I computed the error in each case from the data. Thus I was enabled to obtain determinations without bias; at least I did not know during the series what sort of errors, or how large errors, I was making.

The direction of approach, which Burrow (and also Klemm<sup>1</sup>) have shown to be influential, I controlled by deliberately fixing my attention on a point either prior to or subsequent to the 'apparent region of simultaneity' as shown by the first few revolutions of the disc, and with further revolutions moved my fixation progressively from that point until satisfied. If my first determination was from a point in advance of my final fixation point (labeled 'prior' in the tables) the next was from a 'subsequent' point and so on alternately.

TABLE IV.

RATE 1.25.

Series.	Subsequent.	m.v.	Prior.	m.v.	Average.
16	+6.30	3.92	-2.75	4.6	+1.8
17	+3.45	2.91	-5.8	3.95	-1.17
18	+0.1	2.25	-7.3	3.4	-3.6
19	+0.42	3.6	-7.85	1.96	-3.72
20	+1.8	2.89	-4.5	8.83	-1.35
Averages	+2.42		-5.64		-1.61
$\sigma$	+8.39		-19.57		-5.58

The greater irregularity of the results of this experiment as compared with the results of the first set is perhaps due in part to the heavy threads on the netting at 8 cm. intervals. For the first set the single fixed mark was midway between two of these heavy threads, and so they produced no disturbing effects; but in the second set, as the final index position at the time of the discrete stimulus varied around the dial, the positions emphasized by the threads were possibly influential.

Reverting now to Burrow's method, using the dial with a single fixed mark, I made another set of experiments, starting

<sup>1</sup> Klemm, O., 'Versuche mit dem Komplikationspendel nach der Methode der Selbsteinstellung,' 1907, *Psychol. Studien*, II., pp. 324-357.

in the determinations from 'A' and 'B' alternately as in the first set, but keeping the disk in a fixed position with regard to the contact arm. This relative position of the disk and arm was not noted until after the series was completed, and my readings were taken exclusively from the brass scale of the finger-lever.<sup>1</sup> Results of this set of experiments are given in Table V.

TABLE V.

RATE 1.75.

Series.	'A.'	m.v.	'B.'	m.v.	Average.
21	-0.37	3.65	-8.05	3.6	-4.21
22	+3.65	2.58	-2.77	2.85	+0.44
23	+2.10	3.68	-3.77	4.54	-0.83
24	+1.52	3.53	-3.40	3.55	-0.93
25	+7.50	3.05	+2.1	2.58	+4.8
Averages	+2.88	3.29	-3.17	3.42	-0.14
$\sigma$	+13.99		-15.40		-0.68

The remarkable thing about this set is that while the first series shows a rather large negative error and the last a rather large positive error the whole set averages less than 1 *sigma* constant error. It would have been interesting to take more series, to see if the positive error persisted, but of course I did not know the nature of the results until later, when I worked them over.

The averages are really modified by several unusual negative errors (one of 19°) in series 21, and several exceptional positive errors in 'A' of series 25. My notes written at the time of the experiments specify the set as especially difficult and unsatisfactory, fixation being hard to keep, so that it is probable that judgments of a type different from that of exact fixation crept into these two series. Concerning the formation of averages I shall have something to say later.

To see what my error would be with reversion to the natural fixation method I had employed in Burrow's experiments, I removed the net and took a fourth set of determinations, manipulating the apparatus practically as in the third set, *i. e.*, reading from the brass scale; and using the easiest rate, namely, 1.25.

<sup>1</sup>Thus, in this experiment, as in the preceding one, I did not know what errors I was making.

On the first day my tendency was to let the pointer blur, exactly as when using the net fixation. I allowed a week to go by without further experiment, and then took up the work again, when, without any effort except the attempt to get satisfactory judgments, the pointer came out distinctly at the apparent moment of the click. I proceeded then with the series, the results of which are given in Table VI.

TABLE VI.  
NATURAL FIXATION. RATE 1.25.

Series.	'A.'	m.v.	'B.'	m.v.	Average.
26	+15.12	3.26	+10.72	3.82	+12.92
27	+14.67	2.06	+7.17	2.66	+10.92
28	+17.05	1.97	+12.90	5.32	+14.97
29	+13.42	4.92	+7.67	4.03	+10.55
30	+13.2	2.56	+5.85	3.54	+9.57
Averages	+14.69		+8.86		+11.78
$\sigma$	+50.97		+30.74		+40.87

The results for the third set of experiments show a surprising change in the error as compared with my determinations when subject for Burrow. My errors without the net were originally negative: now they fall on the positive side. This positivizing tendency had shown in the last series given in Table V. which indicates the correctness of my surmise that some judgments by the natural fixation method had crept into that series.

## II. EXPERIMENTS ON OTHER SUBJECTS.

My experiments show quite clearly that with adequate fixation the average displacements in the complication experiment and similar experiments becomes practically inconsiderable. The netting simply gave me the means of knowing, and consequently of guarding against a blurring of a stationary object,<sup>1</sup> which was not so easily noted with the solid black fixation mark.

In October and November, 1909, I repeated the experiment on Dr. Watson, who had not previously served as subject with the complication apparatus or any similar device. He was aware of the results of my experiment on myself, and of the trend of

<sup>1</sup>On this point, however, see Dodge, R., 'An Experimental Study of Visual Fixation,' 1907, *PSYCHOL. REV.*, *PSYCHOL. MONOGRAPHS*, Vol. VIII., p. 55.



my conclusions, and therefore was from the start observant of his fixation. His effort was explicitly to fixate the stationary mark as accurately as possible, and in this effort he soon succeeded admirably.

In the first experiments, without the net, I obtained from Dr. Watson each day twenty-four judgments, twelve with 'A' approach, and twelve with 'B' approach. For each side there were three starting positions of the lever, viz., 40, 50 and 70 degrees from the center of scale; and four starting points for the discrete stimulus with regard to the pointer position, viz., 30, 40, 50 and 60 degrees from the stationary mark. The twelve combinations of these variables were given according to a fixed scheme, so that the extents of movement of the lever, and the positions in which it was stopped, seemed to the subject entirely haphazard.

When we came to experiments with the net, the subject found the eye-strain severe, and was unable to complete twenty-four judgments at one session. The number was accordingly reduced, as indicated in Table VII., which gives the results of these experiments with Dr. Watson. The net was used for series 10, 11, and 12 only.

TABLE VII.

SUBJECT W. RATE 1.25.

Series.	No.	'A.'	m.v.	'B.'	m.v.	Average.	$\sigma$
1	24	-8.66	3.9	-25.75	2.4	-17.20	-59.68
2	24	-5.08	5.8	-13.16	7.5	-9.12	-31.64
3	24	-2.91	3.7	-4.75	5.0	-3.83	-13.29
4	24	+6.25	3.9	+2.16	5.3	+4.20	+14.57
5	24	+4.33	1.7	+1.75	2.9	+3.04	+10.54
6	24	+3.41	1.9	-2.25	2.7	+0.58	+2.01
7	24	+0.16	3.8	+0.41	3.5	+0.29	+1.00
8	24	+5.66	2.2	+2.33	3.8	+4.00	+13.88
9	24	+2.41	3.1	+1.16	2.9	+1.79	+6.21
10	22	-1.90	1.9	+0.63	2.3	-0.63	-2.18
11	18	-1.00	2.6	+3.11	3.6	+1.05	+3.65
12	12	-1.83	2.8	+2.16	1.7	-0.16	+0.54

The results of the experiments with subject *W* are given in degrees, because the spatial measure of the displacement is really more significant than the temporal measure, as we shall see later. For convenience, however, the last column gives

the averages of each series in *sigma*. The second column gives the number of determinations in each series, half being 'A' and half 'B.' Series 1 to 5 show a rapid reduction of an originally negative error, and then the change to positive, as described by Angell and Pierce.<sup>1</sup> From the sixth series on, the subject expressed himself as getting very satisfactory fixation, except in the eighth series, where he said he 'couldn't tell what he was doing.' The results agree with the introspective report. If we average the averages for series 6, 7, 9, 10, 11 and 12, we find a displacement of +0.58 degree, or +2.01 *sigma*. The net made practically no difference in this set of experiments, except to add to the eye strain of the subject. Not only was he subjectively certain that his fixation at the moment of the click was as steady without the net as with it, but the fact that the pointer blurred in exactly the same way in the one case as in the other proved that his conviction was well founded.

I next carried on some experiments with the subject who is designated subject III. in Burrow's report, and to whom I shall refer by that title hereafter. His errors had always been positive, ranging from 17 to 33 degrees. I subjected him to the experiment with the net, with instructions to fixate as steadily as possible. On the first two days he professed inability to maintain the fixation, or at least lack of confidence in his fixation, and the results were about as formerly. On the third and fourth days his fixation was more satisfactory subjectively, and the following series were obtained (Table VIII.). This subject

TABLE VIII.

SUBJECT III. RATE 1.25. VALUES IN DEGREES.

Series 1.		Series 2.		
'A.'	'B.'	'A.'	'B.'	
0	+8	+6	-10	
-2	+3	+9	+17	
-4	+6	-2	+3	
-2	-3	+2	-1	
+4	+8	-3	-5	
+8	-2	+16	+8	
Average	$+0.66 + 2.0$	$+3.33$	$+4.66 + 4.16$	$+3.66$

<sup>1</sup> Angell, J. R., and Pierce, A. H., 'Experimental Research upon the Phenomena of Attention,' 1892, *Am. Jour. of Psychol.*, IV., 528-541.

found the eye strain severe, and the two series given were as long as could be carried through on these two days. The table gives the actual determinations, six 'A' and six 'B' on each of the two days.

It is evident that there are two classes of judgments represented in the second series above presented; one corresponding to the former results of Burrow's experiments with this subject, and one corresponding to the results obtained by subject *W* and myself with exact fixation. Leaving out the + 16 and + 17 from this series gives averages of + 2.4 and + 1.0, and a combined average of + 1.7, agreeing practically with the first series. Compared with the former errors made by this subject — averages from 17 to 33 degrees according to Burrow — these series are significant.<sup>1</sup>

Burrow's subject V. and four other subjects, who were entirely inexperienced, were tested briefly on the experiment. First, they were allowed to make judgments with natural fixation, and all made large errors — over fifteen degrees at the 1.25 rate in all cases, for the 'green' subjects, two of them being of the 'negative' type and two of the 'positive.' Subject V. made errors of slightly less magnitude (see Table I., Sound, in Burrow's paper). On being interrogated as to the perception of the pointer at the critical moment, care being exercised not to prejudice their answers, they all described a more or less definite impression, in cases when the judgment was of simultaneity and was satisfactory subjectively. In the cases of S1 and S3 slight rhythmic twitches of the eyelids were observable, following the rhythm of the click, and in the case of S2 slight *recovery* movements of the eye (movements in the direction opposite to that of the pointer movement) about every third click.

After these series by the natural method, the net was placed over the disc and stationary mark, and the subject was instructed as to the importance of exact fixation. All subjects now noticed

<sup>1</sup> All judgments by this subject which give an error of over 5 to 7 degrees are probably by the natural fixation method. After setting the stimulus with a larger error, the subject was able to correct it to within the above limits by squinting. It is characteristic of the experiment that if the fixation is lost during one or two rotations, it is usually lost for the remainder of the determination.

the lack of a clear impression of the pointer, and the judgments became more irregular, as well as more difficult.

A fifth 'green' subject, S5, found the pointer a mere blur at the critical moment, from the beginning of the experiment, and the net made no difference subjectively nor objectively. Her judgments, after the first half dozen, were distributed through the interval from  $-5^{\circ}$  to  $+5^{\circ}$ .

A sixth 'green' subject, S6, squinted through lids almost completely closed, and made remarkably good judgments from the first; better than S5. I tried this method, but had no success, as the muscular success effort was too distracting. Subject V. tried it quite successfully, however, as did subject III. As to the physiological effects of squinting, I can make no inferences.

### III. THE TYPES OF JUDGMENT.

Exclusive of the judgments made by actually pursuing the pointer through some considerable portion of its circuit, we are forced to conclude that there are two distinct types of judgment, and these comprise two subtypes.

1. *The Exact Fixation Type.* — In judgments of this type the eye is practically at rest at the critical moment, although it is not necessarily stationary for any considerable length of time. (a) *Spatial subtype:* In these cases the judgment is specifically as to the approximate position of the pointer at the moment of the discrete stimulus. There is no time-judgment; the moment of the click and the moment of the alignment are not located in any series, nor with regard to each other. The sound sensation 'strikes out' in consciousness the visual sensation simultaneous with it, with an accuracy far in excess of any possible temporal discrimination. The pointer is however not a distinct positional (spatially) image at the moment of the click, but a blur extending over an area greater than the normal retinal image of the pointer. Theoretically, the judgment should be satisfactory when the front edge of the blur is approximately abreast of the stationary mark, but practically we should not expect a subject to get down to such accurate discriminations. Individual predilections for certain characteristic

relations of blur and stationary mark may also be expected to produce slight constant errors.

Spatial exact-fixation judgments are difficult for most subjects; the strain in attempting to produce eye-steadiness at the proper moments is considerable, especially in the presence of a tendency to react rhythmically, as described below, and the evaluation of the relative position of blur and fixed mark requires intense concentration. In any case, a shift of several degrees in the position of the pointer may be made without seriously injuring the apparent simultaneity.

(b) Temporal subtype: In these cases, the judgment is as to the occurrence or non-occurrence of a time-interval between the discrete stimulus and the approximate alignment of the pointer with the stationary mark. Judgments of this type do occur, as judgments of non-simultaneity, when the discrete stimulus is temporarily remote from the discrete stimulus, as at the beginning of a determination by Burrow's method. But the irresistible tendency to note the position of the pointer at the moment of the discrete stimulus, if it is spatially near, prevents this form of judgment from entering effectively into the final determination; at least my own introspection and that of other subjects leads me to this conviction, although it must be admitted that introspection on these points is not very reliable.

That the temporal form of judgment allows greater latitude for error than does the spatial is clearly brought out by some experiments which I will now describe. These experiments also exhibit the general symmetry of the errors about the point of actual equality, *i. e.*, the lack of a constant error.

The disc bearing the index-hand was removed from Burrow's apparatus, and replaced by a disc bearing a radial slot 4 mm. wide which extended from the edge 50 mm. inwards. The semi-circular dial was replaced by another which was slightly below the level of the disc, and extended under it by more than the depth of the slot. On this dial was placed, on the middle radius, a black strip 45 mm. long, 2 mm. wide at the inner end, and 5 mm. wide at the outer end. The strip was so placed that the wide end projected slightly beyond the edge of the disc into the view of the subject: the re-

mainder of the strip was consequently exposed only during the passage of the slot across it. An electric light placed at the outer edge of the dial, but shaded from the eyes of the subject, threw a good light on the dial beneath the disc. This arrangement provided the conditions for a pure temporal judgment, the black strip appearing before, at, or after the click of the telephone, and the temporal position admitting of variation by the moving of the finger-lever as in the previous work. These conditions are much more satisfactory than with the combination of a flash of light with a sound, for with the black-exposure apparatus the duration of the image of the black mark is positively terminated by the portion of the white disc following the slot.

All the subjects employed on this experiment, except S5, made large errors with this apparatus; much larger than by the natural fixation method with the pointer-disc; and both positive and negative. But actual simultaneity never failed to be perceived as such. I therefore proceeded to find how far the click could be separated from the exposure, in each direction, without the subject losing the perception of simultaneity. Setting the disc for simultaneity with the finger lever at zero on the brass scale, I instructed the subject to move the lever to the right until the click and the exposure appeared non-simultaneous, and then back until simultaneity was just reached. This being done, and the position recorded, the finger lever was returned to zero, and the subject was instructed to repeat the determination in the other direction.

With the exception of Dr. Watson, the limits of the subjects were symmetrical with respect to the actual simultaneity point, and very regular, *i. e.*, were repeated in succeeding determinations almost exactly. The limits for subjects II. and V. were 20-22 degrees in each direction; for myself, 10-12 degrees, for S6, 20-21 degrees, and for subject S5, 3-5 degrees. Dr. Watson proved an exception to the rule as regards symmetry and regularity, his limits running from 20 to 30 degrees with exposure-click and from 10 to 16 with click-exposure. He reported however that the observation was easier with click-exposure than with the other order.

The general symmetry of the indifference-zone in this pure time experiment agrees with the results obtained by Wundt and by Geiger<sup>1</sup> with the complication-clock when they covered the dial except along one radius, at which the pointer was exposed in passing. But the experiment has more importance than Geiger is disposed to admit, since it shows some results obtained by Weyer<sup>2</sup> in measuring the time-threshold between visual and auditory sensations have no bearing whatsoever on the complication experiment, although a mistaken impression as to the significance of these and similar results seems to have been of strong influence on the apperception or 'prior entry'<sup>3</sup> theory of the displacement in the complication experiment.

Weyer found that the time-threshold between a visual and an auditory stimulus depended on which stimulus preceded the other, and on which the attention was fastened predominantly. The threshold was much greater when the visual stimulus preceded the auditory than when the reverse order occurred, other conditions remaining practically uniform. If, when the light came first, the attentive emphasis was on it, the threshold was only about half as great as when the attention was fastened on the sound. When, however, the light came last, attention predominantly to it increased the threshold.

Since the attention to the first member of the pair shortens the time-threshold, and attention to the second lengthens the threshold, exactly as if the interval for any given separation were subjectively lengthened in the first case and shortened in the second, it is easy to assume that the sensation strongly attended to gets more quickly apperceived than the other, and hence the subjective lengthening or shortening of the interval actually occurs. From this point, application of the results to the explanation of the complication experiment phenomena is a simple matter. The prevalent negative error becomes the expression of the greater readiness of 'apperception' to seize on auditory stimuli sensations rather than visual and especial attention to the visual factor, either by voluntary effort or through

<sup>1</sup> Geiger, M., 'Neue Complicationsversuche,' *Philos. Studien*, XVIII., 398.

<sup>2</sup> Weyer, E. M., 1900, *Philos. Studien*, XV., 67-138.

<sup>3</sup> Titchener, E. B., 'The Psychology of Feeling and Attention,' Lecture VII.

some added interest, may overcome this apperception-differential, and produce a zero, or even a positive error.

As a matter of fact, conditions in the spark-snap experiments of Weyer, in which a flash of light appears on a dark background, are quite different from those in the complication-experiment, and from those in the combination of click with exposure of a black strip, as described above. In this latter case, it makes no difference whether the auditory stimulus precedes the visual or *vice versa*, and shifting the attention seems to produce no effect so long as enough attention is given to both to make the judgment possible. Moreover, Weyer found that the time threshold for two successive flashes was from 25.3 $\sigma$  to 49.7 $\sigma$  for flicker, and from 42.6 $\sigma$  to 105.9 $\sigma$  for complete separation, whereas with the exposure apparatus described above the black strip appears sharply separating the preceding from the following white, when the extreme duration of the exposure is less than 5 $\sigma$ !

The greater threshold for light-sound than for sound-light under Weyer's conditions depends of course on the greater persistence of the visual sensation.<sup>1</sup> As for his findings with regard to attention, we may summarize them as follows: In a sequence of two sensations, one of which has a longer duration than the other, attention to the longer emphasizes its beginning if it is the first, and its latter portion or approximate ending if it comes last. That it makes any difference whether the two sensations are of the same or of different senses, no one has yet taken the trouble to find out.

The exact fixation type of judgment, in either the spatial or temporal form, is characterized by the fact that actual simultaneity is never perceived as non-simultaneity. It shows therefore a genuine indifference zone, in which lies the point of actual simultaneity, and about which, in the temporal form, it is symmetrical. The reaction or natural fixation type of judgment, we shall see, shows a *quasi* indifference-zone, with the point of actual simultaneity lying in general outside of it.

2. *The Natural Fixation or Rhythmic Reaction Type.*—When the eye is not obviously following the pointer, and yet a

<sup>1</sup>Wundt, *Physiol. Psychol.*, 5th ed., Vol. III., p. 65.



tolerable image of the pointer is obtained at the critical moment, there must be some sort of activity which obtains this image, since with the optical apparatus in an actually static condition the pointer would be reduced to a blur at all but the slowest rates. We are justified in speaking of a reaction as the producing cause, although we may not be able to specify the exact nature of this reaction. The activity in question may be, and it seems probable that it is, a slight movement of the eye in the direction of the pointer, at the critical moment. This reaction is an involuntary movement, guided by the rhythm of the discrete stimulus and the movement of the pointer. That it may be very slight, and yet produce its effect in dissociating the pointer from the blur, may be shown by the following experiment.

A disc, composed of eight or more equal sectors alternately black and white is rotated behind a double cross made of four narrow black strips, enclosing a small square at the center of the disc. The strips are brought as close as possible to the disc without rubbing. The speed of rotation is such that the sectors just blur. On allowing the eyes to wander across the disc, a portion of a white sector will occasionally flash out, and sometimes a sector will appear sharply cut across by one of the black strips. The best results are obtained by having fixation marks at the right and at the left of the disc, and changing the gaze from one to the other without attending to the eye-movement.

The appearance of the sector cut cross by the strip, can be explained only as due to the simultaneous impression of the two on the retina. If the retinal impressions were received in succession on the same area, the image of the white sector would cut across the black strip, instead of being interrupted by it. It is evident therefore that the angular distance traveled by the eye while receiving the image of the pointer effectively, is very small, for the simultaneously received impression of the stationary strip does not blur. That the impression of the sector is due to a momentary correspondence of eye movement and movement of the seen portion of the sector, and not to an alternation of periods of visual sensitivity with periods of visual anesthesia,

is apparently proved by the fact that the sector seen is always one which is moving in the direction of the eye-sweep; on the bottom of the disc therefore for one direction of eye movement, on the top for the other: and by the fact that when the sector flashes out no portion of the double cross is visible except the portion that happens to lie across the sector.

How slight an amplitude of movement of the eye would produce the flashing out of the sector cannot be estimated with any closeness. I made some rough experiments by dropping the disc, the eye remaining fixated on an object placed vertically above the center of the disc. With the eye three meters from the disc, a drop of 0.7 cm. always brought out the inner half of the sectors on the upward-moving side of the disc (the outer portion was moving too fast to be brought nearly to rest by the slight velocity of the drop). Since this drop is equivalent to a movement of the anterior surface of the eye of less than  $3/50$  mm., and since the latter part of the drop of the disc was alone effective (the velocity in the earlier part being too low), it is evident that a movement of the eye not only too slight to be seen by another observer, but even too slight to be recorded by any of the photometric methods now in use, would be sufficient to bring out the image of a moving object at moderate rate, if the eye movement was in the direction of the object's movement, and at a rate near that of the object.

Whether the reaction by which the cerebro-retinal image at a given moment is differentiated from the images preceding and following, is an eye-movement, or some other activity, may be left an open question. I shall treat it for the present as if it were an eye-reaction, since all analogies point in that direction.

After the rhythm of the discrete stimulus and of the pointer-rotation is definitely established, the subject of the complication-experiment who uses natural fixation notes only one position of the pointer in each round, the reaction being made either with the discrete stimulus, or, in exceptional cases, at the alignment regardless of the discrete stimulus. If the pointer is caught approximately at the alignment with the stationary mark the click will seem simultaneous with that position of the pointer, if it does not come too long before or after; in other words, the

judgment is in so far on a temporal basis, although the error may be regulated by non-temporal factors.

If the pointer is caught at some other than the fixated point, with no appreciable time-interval between this catching and the discrete stimulus, this occurrence is taken as a sign of non-simultaneity at the fixated position, and accordingly, by Burrow's method, the temporal position of the discrete stimulus is altered, or, by the classic method, the fixation point is changed.

Whenever the reaction occurs within the limits of the time threshold before or after the discrete stimulus, which limits are variable, but may be safely said to be less than one hundred *sigma* in ordinary cases, the discrete stimulus will seem to occur at whatever point in the path of the pointer happens to be that at which it is caught by the reaction. The determinant of the error will therefore be the lack of coincidence of the reaction with the discrete stimulus.

Voluntary rhythmic reactions, *i. e.*, reactions to a stimulus repeated in regular rhythm, may be of two sorts. The reactor may attempt to make the reaction, or a definite part of the reaction, synchronous with the stimulus, or he may attempt to make it follow or precede the stimulus.

It is generally supposed that synchronizing rhythmic reactions may be made with surprising accuracy, although subject to some irregular variation. For the purpose of observing such reactions I modified my apparatus in a new way. I suspended a helium tube just above the fixed mark on the scale (in practically the position of the Geissler tube in Burrow's experiment with light), so that when the tube lit up it illuminated both stationary mark and the adjacent portion of the disc. This tube was connected in the secondary circuit of a large induction-coil, and a break key was inserted in the primary circuit. The room was darkened so that as the subject reacted by depressing the key, the position of the pointer with regard to the stationary mark was distinctly visible to the experimenter at each reaction. The telephone-click, as described above, was employed for the stimulus, the telephone being placed close to the ear of the subject as he sat at a table with the reaction key before him. The disc was so adjusted that the click came exactly at the

alignment of pointer and stationary mark, so that the error in the reaction could be read off directly in degrees.

Nearly all of the subjects required a half hour of practice before they succeeded in reacting to the rhythmic stimulus in a manner satisfactory to themselves. Four subjects, who regularly made negative errors in the complication experiment, anticipated almost exclusively in the reaction. The anticipation, in cases where the reaction and stimulus seemed to the subject simultaneous, was in the majority of cases within 20 degrees of actual simultaneity at the 1.25 rate, but larger errors were frequently perfectly satisfactory to the subject. Very seldom did these subjects hit within 5 degrees of simultaneity, and only occasionally was the reaction delayed.

Subject III. always began a series of reactions with five or six delayed reactions, changing then to anticipatory, and reverting sporadically to the delayed type. Taking his series as wholes, the delayed reactions numbered about one to two anticipatory reactions, the magnitude of error being not much different from that with the four subjects just mentioned. The errors of this subject in the complication experiment, it will be remembered, were positive.

Dr. Watson anticipated as a rule, and the errors in some of his series came under 20 degrees at the 1.25 rate. In some series, however, the reaction was premature by from sixty to fifty degrees, and yet seemed to him to synchronize perfectly with the click. Occasionally the subject delayed, but the ratio of delayed to anticipatory reactions was not higher than one to ten. Within series of from twenty-five to forty reactions, Dr. Watson was more uniform than the other subjects; frequently I counted eight to ten reactions in succession which did not vary more than two or three degrees from each other.

I myself usually anticipated, but on some days delayed almost exclusively. Judging by the reports of the various persons who observed my reactions, my errors were about like those of the first four subjects mentioned above.

Most of this work was done with the one rate of 1.25 sec. per rotation. I tried other rates, from 2.0 secs. to 0.71 sec., but found no essential difference in any case; the amounts of

the errors in degrees changed perceptibly with the rate, but as nearly as I could estimate the amount in time remained the same. The key used broke contact at the bottom of the stroke; a key which broke contact at the beginning of the movement gave exactly the same results with four subjects on whom I tried it.<sup>1</sup>

After satisfying myself as to the subjects' performances in attempted synchronizing reactions, I instructed them to fall in behind the click; to wait on the stimulus as it were, but to attempt to follow it with the least possible intervening interval.

After a little practice, all of the subjects except two found the follow-reaction easier and more satisfactory than the synchronizing, and their errors were actually much less, being uniformly delays. Of the two subjects who had difficulty with the follow-reactions, one made smaller errors and the other larger than in the synchronizing reaction.

The results of these rough preliminary experiments agree with the statement of Scripture<sup>2</sup> that almost all subjects anticipate in rhythmic reactions, and agree also with the usual negative error with the complication experiment. The greater ease of the delayed reaction throws light on the tendency of the error to become positive after habituation to the experiment.<sup>3</sup> Certain reactions giving the subject more satisfactory judgments than do others, he gradually falls more and more into the habit of that way of reacting.

More extensive investigations into the rhythmic reaction may show an influence of rate corresponding to the effect of rate upon the complication judgments. Certain rates will undoubtedly be found which will call forth the maximum of accu-

<sup>1</sup>The effects of practice with and without orientation offer a series of problems whose solutions are of the highest importance. Certain of these problems it is my intention to take up in the near future, with apparatus recording the errors instead of merely making them visible. In this connection a brief experiment with the apparatus described, with a subject experienced in astronomical observation, and hence with preceding oriented practice in the following of rhythmic stimuli, is suggestive. This subject, at the rates 1.0 and 0.6 made errors ranging from 0 to 5 degrees, about as many delayed as anticipatory. On the complication experiment, his errors were of approximately the same sort: positive and negative indifferently up to 5 degrees.

<sup>2</sup>Scripture, E. W., 'Observations on Rhythmic Action,' 1899, *Yale Psychological Studies* VIII., p. 103.

<sup>3</sup>Angell and Pierce, *op. cit.*, p. 534. Geiger, *op. cit.*, pp. 358-364.

racy for any given subject, and possibly the error will be found in other respects to depend somewhat on the rhythm.

The reaction in the complication experiment, especially after fair habituation to the conditions, is normally to the discrete stimulus, although the rhythmic movement of the pointer is a disturbing factor, especially at first. The limits of error are therefore the maximal anticipation and delay in the reaction, since these are in general less than the least observable time differences between reaction and stimulus. This latter is shown by the fact that normal error in the rhythmic finger reaction is less than the errors which occasionally occur without being noticed, and also by the fact that the limit of error in the pure time-judgment experiment (the black strip exposure experiment) is for all but one subject greater than the errors of the same subject in the complication-experiment.

If, after the subject in the complication-experiment has found a position for the discrete stimulus which gives a satisfactory judgment of simultaneity, but with error, the stimulus is moved *nearer* to the actual alignment, the judgments on this new position will in general not be of simultaneity, because the reactions will not now catch the pointer at alignment. Some slight variation is possible, because the pointer is never caught with absolute sharpness.

Reactions to the pointer, instead of to the discrete stimulus, are possible, but are irregular and unsatisfactory, as each variation in the error changes the rhythm. With the finger reaction device described above the subject, if placed in the normal position before the apparatus for the complication-judgment, could flash the pointer out repeatedly near the stationary mark, without the aid of the click. The errors were of the subjects' usual types, but very erratic. In the complication-experiment judgments by reactions to the rhythmic passage of the pointer would have as limits of error the maximum imperceptible time between the reaction and the discrete stimulus. In the case of a judgment giving error the discrete stimulus might be placed nearer to or at the actual alignment and still the appearance of simultaneity be preserved. In general, as above stated, this substitution cannot be made in the case of a determination by

the natural fixation method ; which shows that the pointer-reaction practically does not figure in the final judgments by this method. Yet in Burrow's method such reactions do occur at the start of a determination, because then the pointer is too far from the fixation mark to be clearly seen at the moment of the discrete stimulus. This is shown by the introspection of several subjects on the judgment directly ; that the judgment was of time elapsed between the discrete stimulus and the alignment ; and also by the practically unanimous report that large shifts of the lever produced relatively slight effects at first — shortening a long time interval produces an effect disproportionately small. These judgments apparently did not occur after the click came near to the alignment, and hence Burrow's attempt to get determinations by the temporal method alone<sup>1</sup> proved a failure.

James' theory<sup>2</sup> of the time displacement ; that it is due to the inability of attention to deal with the impression of the pointer and that of the discrete stimulus at the same time, because of which inability it attends to them in succession, and the time interval not being perceived, the succession is mistaken for simultaneity ; might be modified to suit the reaction hypotheses. We might suppose that the subject is unable to attend to other than the visual stimulus at the time of the reaction, hence requires a succession such that he can attend to the discrete stimulus just after or just before the reaction. Judgments without error would be supposed to be due to the use of some other than the reaction method of observation. This explanation of the time-displacement is excluded by the results of the following experiment.

Allow a subject to make a determination in the complication-experiment, using the natural fixation method. If he makes a considerable error, arrange a second contact point, to produce a second click, exactly at the alignment at which the subject thinks the first click comes. Call the first click  $C_1$  and the second  $C_2$ . Now both clicks being heard in succession, the subject, if his

<sup>1</sup> Burrow, *op. cit.*, pp. 54-55. I do not agree completely with Burrow's explanation of the 'spatial-temporal' distinction, and I do not think it corresponds to the 'naive-reflecting' classification of Geiger.

<sup>2</sup> James, W., *Principles of Psychology*, I., p. 415.

original error was positive, will judge that  $C_2$  is correct, and  $C_1$  is incorrect. Cut out  $C_1$ , and  $C_2$  seems too late; cut out  $C_2$ , and  $C_1$  seems correct. With both clicks (it is safe to conclude), the subject, reacting to the first click, catches the pointer at the alignment, whether the second click occurs or not. If the second click occurs, since it comes exactly at the perceived alignment, it is perceived as simultaneous therewith, and the first click seems therefore too soon. If  $C_2$  alone is given, the subject usually reacts to that as he had previously to  $C_1$ , and therefore will not catch the pointer at the alignment.

With subjects making a negative error the experiment does not always succeed. Here  $C_2$  comes before  $C_1$ , and there is a tendency to react to it. But if  $C_2$  is cut out, by means of a key in the circuit, and the subject is allowed to observe for several rounds of the pointer, thus getting the rhythm of  $C_1$  well established, and if  $C_2$  is then suddenly thrown in, the subject will usually go on reacting to  $C_1$  for a few rounds, getting  $C_2$  therefore in simultaneity with the alignment.

If the double-click experiment is applied to a subject making an error by the exact fixation method, the second click will appear correct, and will remain so when the original one is cut out. This was notably the case with Dr. Watson, and sharply characterizes the exact fixation method. The test is however applicable only occasionally to subjects of this type, since the errors are usually too small to allow of the insertion of the contact for the second click at exact zero.

There can be no question, it seems from the above described experiment, that the subject is perfectly well able to attend to the discrete stimulus at the moment of the alignment. The source of the errors lies in his inability to react so that the pointer is caught at the moment of the stimulus. By the reaction method, as by the other methods, simultaneous impressions of the pointer and of the sound *are always perceived as simultaneous*, but impressions not simultaneous are also perceived as simultaneous, if the time discrepancy is not too great. The error is due to the fact that the subject, on account of his peculiarities of reaction, does not get an impression of the pointer at the moment of the discrete stimulus, but does get one just before or just after.



The apperception explanation of the time displacement, which assumes that impressions actually simultaneous seem non-simultaneous, is completely discredited by the above facts. Such an illusion has been shown to occur in the case of two stimulations of which one lasts longer than the other, when both begin at the same moment. In that case the illusion has a definite direction, not reversible, although the extent is variable. In other circumstances no illusion of time-displacement has been demonstrated. *In the complication experiment there is no illusion of time-displacement; the displacement is real, and the fallacious perception is of simultaneity.*

Some observations made with an artificial pupil seem significant although their exact bearing is not yet apparent. If the artificial pupil is moved to one side, the other side of the disc (*i. e.*, the right side, if the pupil is moved to the left) may be hidden. If the left side of disc and dial were hidden in this way; except for a narrow strip adjacent to the stationary mark, and to the pointer when in alignment with the mark; the experienced subjects (Dr. W., subject III., subject IV., and myself) made positive errors exclusively, while the 'green' subjects all made negative errors. (The pointer rotated from left to right in passing the mark; *i. e.*, clockwise.) With the right half of the field covered, the errors were for all cases the reverse of the errors with the left half of the field covered. Covering the field by a cardboard just above the level of the disc had no effect on the direction of the error in any case. The fact that in this latter case movements of the eye had practically no effect on the extent of the visual field, while with the artificial pupil a slight movement in one direction cut off the entire field, and movement in the other direction enlarged the field, may shed some light on the phenomenon.

3. *The Pointer-pursuit Method.*—Thus far we have assumed that the judgments in the classic work on the complication experiment from Wundt to Burrow were made by the natural fixation method. Burrow's subjects certainly employed this method. Wundt,<sup>1</sup> von Tschisch,<sup>2</sup> and Klemm give no

<sup>1</sup> Wundt, W., *op. cit.*, pp. 67-86.

<sup>2</sup> Von Tschisch, W., 'Ueber die Zeitverhältniss der Apperception einfacher und zusammengesetzter Vorstellungen,' 1885, *Philos. Studien*, II., pp. 603-634.

information as to their methods of observation; Pflaum<sup>1</sup> says that he followed the method of Wundt and von Tschisch. Angell and Pierce<sup>2</sup> always followed the pointer on its first round, and then proceeded to fixate the point so picked out. Geiger<sup>3</sup> found two classes of subjects, whose methods he calls the 'naïve' or 'pointer-observing' and the 'reflecting' or 'scale observing.' He admits that the two methods are not always clearly separable. In observing by the 'reflecting' method the subject practically used natural fixation after the first few rounds of the pointer; as to the fixation during the first rounds we are not accurately informed. By the 'naïve' method, the subject pursued the pointer for the first few rounds, and then employed natural fixation. Geiger's 'naïve' type therefore corresponds practically to the method of Angell and Pierce, although for purposes of argument Geiger classifies them as 'reflecting.'<sup>4</sup>

Since it is possible to follow the pointer at rates up to at least 1.0 sec., and perhaps at still faster rates, it is important to find out what sort of judgments result from this method, that we may know what effect these judgments may have had in the classic experiments.

Angell and Pierce<sup>5</sup> reported that judgments based on the first rotation (the pointer being followed during this rotation), gave invariably positive errors. I wished to check this observation, and also to test the explanation they offer; that the eye comes to a stop after the discrete stimulus, the error measuring the time required for this arrest.

On the shaft of a controllable motor I mounted a white cardboard disc of 28 cm. radius. In this disc I cut a radial slot 5 degrees wide and 15 cm. long, extending from a point 8 cm. from the center of the disc to a point 5 cm. from the edge. This slot was covered with white tissue paper pasted on the back of the disc. Behind this disc was placed a stationary disc of

<sup>1</sup> Pflaum, C. D., 'Neue Untersuchungen Ueber die Zeitverhältniss der Apperception einfacher Sinneseindrücke am Complicationspendel,' 1900, *Philos. Studien*, XV., pp. 139-148.

<sup>2</sup> Angell and Pierce, *op. cit.*, p. 533.

<sup>3</sup> Geiger, *op. cit.*, p. 374 *et seq.*

<sup>4</sup> Geiger, *op. cit.*, p. 384.

<sup>5</sup> Angell and Pierce, *op. cit.*, p. 534.

black cardboard, having in it a hole about 14 mm. in diameter, 18 cm. from the center. This disc slipped over the bearing of the motor-shaft, and could be turned to bring the hole in any radius, being held by clamps from the side and top when in position. Directly back of the hole was placed a 16 c.p. electric light. The revolution of the slotted disc therefore exposed progressively a roundish area of yellow light as the slot passed in front of the hole, the slot being much narrower than the hole. Perhaps better results would have been obtained had the tissue paper covered the hole instead of the slot.

In operation, the illumination on the front of the slotted disc was reduced to such intensity that the flash of the exposure of the hole was bright; the face of the disc was still plainly seen, and the slot appeared distinctly as a dark stripe. The subject was seated at a distance of six or eight feet in front of the disc.<sup>1</sup>

At rates of from 0.5 to 2.5 sec. per rotation of the disc, the subject if given no observation instructions, usually sees the flash as a practically round spot, *on the surface of the disc behind the slot*; sometimes as much as 90 degrees behind. The roundness of the spot shows conclusively that the eyes (or eye; the results are the same if one eye is used) are at rest at the moment of the exposure, or at least are not moving fast enough to distort the image; the movement at rate sufficient to obtain an image of the slot coming shortly after the exposure, as shown by the positive error. The reaction is in short delayed.

If the subject begins to reproduce the rhythm of the flash muscularly, the error (*i. e.*, the apparent lag of the spot behind the slot) becomes more irregular, but in general less, and may change into a negative error (*i. e.*, the flash may appear on the disc in front of the slot).

By careful fixation of a mark placed in front of the disc, the error may be practically destroyed. In this case the slot no longer appears distinctly. A narrow strip of black cardboard

<sup>1</sup> I have also used with good results a slotted light-gray disc with a round white spot pasted on a black disc, and a slotted dark-gray disc with a black spot pasted on a white disc. Either of these combinations will give good results, but the round hole illuminated from behind is more satisfactory, because the intensities are easier to adjust, and also because the other combinations must be placed more carefully with regard to the direction of illumination of the room.

supported in front of the disc, and extending radially from the position of the light spot makes a good fixation mark. Perhaps cross-lines would be better. For some reason fixation is easier than with the complication-disc, possibly because the slot is less prominent than the black pointer used in the other experiment.

So far the experiment is a simple and striking verification of Burrow's finding, of displacement with visual discrete stimulus, and also of my findings with regard to the effects of exact fixation. The most important results come out with pointer pursuit.

The subject can easily pursue the pointer (with reservation noted below), at rates covering a wide range. The light-spot now appears no longer round, but as a narrow streak, being in fact the illumination of a section of the slot. There is therefore no displacement of slot as regards the spot, but the illumination seems to be farther along in the path of the slot's rotation than it really is — there is a decided positive error. If a fixed mark is supported in front of the disc, on the radius of the light spot, as described above, the displacement is especially notable.

The explanation of this displacement is undoubtedly given correctly by Angell and Pierce. The eye comes to rest a certain length of time after the occurrence of the discrete stimulus, but the time interval is not noted, and hence the stimulus is assumed to have happened when the eye was in the position in which it comes to rest. In the case of the visual stimulus, the after-image of the flash assists the illusory effect, because it remains in the position in which the stimulus is supposed to have lain. But the conditions would be much the same without any visual after-image. The eye, during true pursuit movements is unable to orient itself, and must stop for that purpose; while moving objects are being pursued, stationary objects are not distinctly seen. In this particular the true pursuit movement differs essentially from the movement which brings out the sector in the experiment earlier described.

Sometimes, the eye, while the subject is attempting to pursue the pointer, comes to a stop before the flash. In this case the flash is *round*; a certain indication. If the eye stops while

the light is being exposed, the spot is D-shaped, the straight edge being in the rear; and if the eye is at rest at the beginning of the exposure, commencing to move before the end, the spot is D-shaped, but the straight edge is in front. In the last two cases, which occur infrequently, there is no displacement. In the case where the eye stops before the exposure, the displacement is in the negative direction, for obvious reasons. Apparently, the eye, if it stops at the exposure, or immediately before it, does not get in motion for some little time afterwards, so that positive errors in these cases are practically out of the question. Positive errors do occur, however, with round spot, while the subject is attempting to pursue the pointer, because the eye stops so long before the flash that the judgment becomes practically of the natural fixation type. In these cases, the subject usually realizes that he is not following the pointer accurately.

This tendency to follow the pointer inaccurately becomes stronger after the first one or two rounds. The first few rounds, the pointer is easily followed, and the slot-shaped spots and positive displacements obtain exclusively. Then, the tendency to run ahead and fixate becomes strong, and the round and D-shaped spots—principally round—begin to occur. The natural tendency is to cease the effort to pursue, and to fixate. Hence, I conclude that Geiger's 'naïve' subjects were of the same type as Angell and Pierce. We may safely conclude that all the classic observations were made by the natural fixation method, tempered in a few cases by preliminary observations by the pointer-pursuit method. How much influence on the final judgment is exercised by the preliminary observations is a question to be settled by further experiment.

#### IV. THE INFLUENCE OF EMPHASIZED POINTS.

Wundt<sup>1</sup> and Geiger<sup>2</sup> noticed that if certain points in the path of the pointer are especially emphasized, as by the placing of marks, or simply by their being the top, bottom, etc., of the path, there is a tendency for the apparent point of entry of the discrete stimulus to be drawn towards such emphasized

<sup>1</sup> Wundt, *op. cit.*, p. 69.

<sup>2</sup> Geiger, *op. cit.*, pp. 384-394.

points. This observation has so far remained a mere brute fact, not provided for by any theory of the complication error, but it is readily accounted for after discriminating properly between types of observation.

By the exact-fixation method, the position of the pointer is indistinct, and it covers a small stretch of path rather than an approximate point. Hence, any fixed point within that stretch is apt to be selected as the locus. If such a point is brought out in any distinguishing way, it is much more liable to be selected, for there is no positive reason against the selection. Hence, in the experiments on Dr. Watson and myself, there shows a constant difference between the 'A' and the 'B' judgments. On the average, the correction will be stopped before it quite reaches perfection, for if there is a slight range of indifference for any setting, within which the emphasis of a point is effective, when the adjustment brings the actual alignment within this range it will be satisfactory.

The indifference-zone displayed in the results of Burrow's experiments owes its existence to a different cause. By the natural fixation method, the perceived position of the pointer is much more exact than by the exact fixation method. Hence the actual range of indifference in any given judgment is relatively small. The emphasis of any point operates here by modifying the delay or anticipation in the reactions on which the judgments are based. If the reaction catches the pointer a little before (or after), a point which is especially prominent, there is inevitably a feeling of prematureness (or lateness) and the succeeding reactions will be slightly more delayed, or less anticipatory (in the one case, and more anticipatory or less delayed in the other).

The distance of the starting point from the actual alignment was found by Burrow (see his Tables III. and IV.) to be very important; the greater the discrepancy at starting the more the error at the final judgment tended in the direction of that discrepancy, or the less it tended in the opposite direction. Evidently, the more distant the starting point from the final setting the more pronounced the effect in increasing or decreasing the anticipation or the delay. This operated in a rather relative

way, the contrast with the longer distances modifying the effect of the shorter. Thus, while the 'A' 75° starting point gives in all cases (except with subject II., for reasons explained by Burrow), a more positive value of the error than the 'B' 75° starting point; the relation is more often reversed than not by the 45° starting points.

The results of my experiment on Dr. Watson show a slight tendency in the same direction as the results in Burrow's tables. The averages for the four starting positions on each side, for 'A' and 'B,' of the series from 3 to 9 inclusive, are given in Table IX. below, in degrees.

TABLE IX.  
STARTING POSITIONS.  
SUBJECT W. SERIES 3-9.

	'A.'	'B.'
30°	+2.09	+1.71
40°	+2.27	0.04
50°	+3.27	-1
60°	+3.38	-0.28

This tendency indicates pretty clearly what I had other occasions to suspect, namely, that certain of the series contained a few judgments by the natural fixation method. This takes us over to the matter of the composition of averages from judgments of different sorts.

#### V. THE MAKING OF AVERAGES.

Although the previous statements with regard to the complication experiment have been based on averages, none of the experimenters has evidenced any concern as to the composition of these averages. Burrow is the only one who has presented the mean variations, and these are large enough to give food for thought. Pflaum, it is true, does give the number of positive and negative judgments, respectively, represented in each average. In Pflaum's results, practically every average is drawn from errors of both types, and in some cases there were two of one kind and one of the other! In other cases there were two of one kind and three of the other, and so on.

With the exceptions mentioned, the literature contains no

details concerning the formation of the averages except the number of determinations. And yet, it is very important to know, in cases where the number of determinations entering into the averages is adequate, whether a decrease (for example) in the magnitude of the average error means that the average *magnitude* of the negative errors decreased, or merely that the *number* of negative errors decreased. Such information is of especial value in cases of subjects who show a gradual change of the average error from negative to positive.

Table X. gives the number of positive and negative errors in each average of Table VI. (subject *W*), and also the average for the errors of each type separately. The figures show an irregular fluctuation in both positive and negative errors, but no progressive change in either positive or negative after the fourth day. In general, from the fifth day on the average magnitudes of positive and negative errors are nearly equal. Table XI. gives the positive and negative averages from my own judgments. In my case the fluctuations are more pronounced. Averages are given in degrees in both tables.

TABLE X.  
NUMBERS AND AVERAGES OF POSITIVE AND NEGATIVE ERRORS FROM  
TABLE VII. SUBJECT W.

Series.	'A.'				'B.'			
	Positive.		Negative.		Positive.		Negative.	
	No.	Av.	No.	Av.	No.	Av.	No.	Av.
I	0	—	12	8.66	0	—	12	17.40
2	3	4.66	9	8.33	0	—	12	13.16
3	3	4.66	9	5.44	2	3.50	8	6.4
4	11	6.81	0	—	5	7.60	5	2.40
5	12	4.33	0	—	8	3.87	3	3.33
6	10	4.7	2	3.30	2	3.50	7	4.85
7	6	2.66	6	2.33	7	3.42	5	3.16
8	12	5.66	0	—	7	4.56	3	1.33
9	9	4.33	3	3.33	7	3.62	3	4.00
10	2	4.0	8	3.12	4	2.50	6	2.80
11	3	2.66	5	3.40	5	6.80	2	2.40
12	2	2.5	4	6.0	3	5.33	1	3.0

The series from 16 to 25 inclusive, of my results (from Table IV.) are listed in Table XI. with the 'subsequent' determinations under 'A' and 'prior' under 'B,' to which of course



they correspond, since the 'A' approach means that the fixation point was 'at first *after* the actual point at which the discrete stimulus occurred.

TABLE XI.

NUMBERS AND AVERAGES OF POSITIVE AND NEGATIVE ERRORS FROM TABLES I-VI. SUBJECT D.

Series.	'A.'				'B.'			
	Positive.		Negative.		Positive.		Negative.	
	No.	Av.	No.	Av.	No.	Av.	No.	Av.
1	7	6.28	2	3.5	0	—	9	8.11
2	3	5.33	5	5.6	4	5.5	6	8.16
3	4	5.0	4	4.75	4	11.0	6	4.33
4	8	7.25	2	7.0	6	9.16	4	10.75
5	4	8.5	6	8.0	5	3.8	5	4.0
6	5	2.8	3	2.66	6	2.5	2	3.5
7	7	2.28	3	2.0	4	3.5	6	4.0
8	4	4.5	6	6.16	3	3.66	5	3.0
9	5	4.8	4	5.0	6	4.0	4	4.25
10	5	3.0	4	1.75	6	2.33	3	4.0
11	5	2.8	5	7.8	2	6.0	8	8.5
12	5	14.5	5	9.2	7	10.14	2	3.0
13	4	6.5	4	3.25	3	8.33	7	8.42
14	5	6.4	5	11.6	4	14.0	6	9.0
15	2	5.5	7	8.0	2	9.5	6	5.83
16	10	6.35	0	—	3	3.16	6	6.16
17	8	4.75	2	1.75	2	1.75	8	7.43
18	5	2.25	4	2.5	10	7.35	0	—
19	3	6.83	5	3.15	10	7.85	0	—
20	8	3.28	2	4.12	2	8.5	7	8.89
21	6	2.66	4	4.93	0	—	10	8.05
22	9	4.83	1	0.25	3	1.25	7	4.46
23	6	5.16	4	2.75	2	3.75	8	5.78
24	5	4.85	5	1.8	2	4.62	8	5.40
25	10	7.5	0	—	8	3.28	2	2.75
26	10	15.12	0	—	9	11.91	1	1.50
27	10	14.67	0	—	10	7.17	0	—
28	10	17.05	0	—	10	12.9	0	—
29	10	13.42	0	—	9	9.0	1	4.25
30	10	13.2	0	—	8	8.71	2	5.62

The variability of the numbers and magnitudes of the positive and negative errors within small limits, and the general tendency of the magnitude of the errors of the one kind to equal that of those of the other, with occasional large exceptions, is nicely shown. The first two series in Table X. and the last five of Table XI. are not included in this statement, because, as earlier explained, they belong under the natural fixation method.

The way in which the number of errors may work against the magnitude of error is illustrated by series 9 and 10 of Table X. The average positive error in 'A' of series 9 is only slightly greater than the average positive error in 'A' of series 10, and the average negative error is also somewhat larger in series 9. But the predominance of the positive errors in 9 and of negative errors in 10 makes the first average positive and the second negative (see Table VII.). On the other hand, there are relatively more positive errors in 'B' of series 5 than in 'B' of series 4; yet the greater value of the positive errors in series 4 gives the average of both positive and negative a more positive value in series 4 than in series 5. Similar effects of composition are to be observed in Table XI. These matters are not so important in work by the exact fixation method, as in the case of the natural fixation method. It is evident that a change in the average error in the latter case can be produced in either of two ways, and the change is not of much importance for explanatory purposes unless we know in which way it is produced.

#### CONCLUSION.

We may consider it as established that there are three general methods of judging the position of a moving pointer at the moment of a discrete stimulus, and that each of these methods produces characteristic results. The pointer-following method produces positive errors, the exact fixation method produces small errors, indifferent as to direction, and the natural fixation method produces errors depending on the retardation or anticipation in the reaction which is instrumental in the securing of an image of the pointer in a certain relative position.

It is safe to say that in the normal subject sensations whose peripheral processes are simultaneously excited are always perceived as simultaneous, unless the duration of one is much less than that of the other, and that sensations of different modality which are excited successively are apt to be perceived as simultaneous even when there is considerable interval between them. The errors in the judgments by the natural fixation method are in general much less than the time-threshold, and may even be zero, if the reaction happens to synchronize perfectly with the stimulus.

There is strong probability that the reaction in the natural fixation method is an eye-reaction. This is a matter for further investigation, and merges in the complex problem of vision in eye-movement.

The most immediate opening for work in connection with the remnants of the complication-problem is upon rhythmic reactions in general. The exact effects of rate, habituation, practice (*i. e.*, habituation with knowledge of results, and attempt to modify results in a certain way), type of stimulus, and type of reaction, in cases where the subject attempts to synchronize with or follow a rhythmic stimulus, must be ascertained.

## THE PENDULAR WHIPLASH ILLUSION.<sup>1</sup>

BY ALGERNON S. FORD.

Professor Dodge, in an article entitled 'The Participation of the Eye Movements in the Visual Perception of Motion,'<sup>2</sup> attaches considerable importance to a certain phenomenon which he terms the 'whiplash illusion.' This illusion is obtained by fixating one of two moving lights which are attached to the two arms of a counter-balanced pendulum. The fixated light is perceived centrally, the non-fixated, peripherally. "When the point fixated approaches its extreme position in each oscillation, it seems to rest for an appreciable interval, while the other point seems to continue moving as though the two were connected by an elastic rod, which regularly gave the unfixated point a considerable additional oscillation after the fixated point had been arrested at the end of each swing. The illusion is persistent and striking, and is capable of only one explanation. It occurs at that part of the pursuit movement which photographic registration shows to be practically free from corrective movements. The fact that the point whose image remains motionless on the retina during an unbroken pursuit movement seems to stand still, while the other point, which is in reality moving no faster than its fixated companion, seems to make a little gratuitous whiplash excursion, serves at once to show the utter inability of the pursuit movement either to subserve the perception of motion of the fixated point or to correct the exaggerated data from the displacement of the retinal image of the non-fixated point."<sup>3</sup> Dodge has given a good description of the whiplash illusion, and he believes that this illusion sustains his theory that pursuit movements do not subserve the perception of motion. No positive theory, explanatory of the illusion, is found in the article in question, and it is by no means a certainty that

<sup>1</sup> From the Harvard Psychological Laboratory.

<sup>2</sup> Dodge, R., *PSYCHOLOGICAL REVIEW*, 1904, Vol. XI., pp. 1-14.

<sup>3</sup> *Op. cit.*, p. 14.

Dodge's claim, that the illusion is 'capable of only one explanation,' will hold after further experimental data have been submitted.

Dr. Harvey Carr, in an article<sup>1</sup> entitled 'The Pendular Whiplash Illusion of Motion,' takes a view opposed to that of Dodge, and suggests two possible explanations of this illusion. His first theory, which we shall for convenience term the *limen* theory, is given below: "As the pendulum approaches the end of its swing, the rate of movement gradually decreases to zero. Consequently, for some definite portion of the end of its swing, its rate would be below the eye movement limen, but still above the retinal limen of perceptibility. In other words, the retinally perceived light would be seen moving for an appreciable time after the fixated light had apparently stopped. Hence the gratuitous whiplash excursion is evident."<sup>2</sup> This positive assumption by Carr is supported by historical opinion. It has been commonly held that eye movements could mediate visual motion for the greater magnitudes and velocities, and that the limen of perception here is higher than the limen for retinal displacement.<sup>3</sup>

Carr gives most attention to his second or *positive after-image streak* theory. When the fixated light is followed properly, no positive after-image streak is appreciable, at least for the last portion of the swing. Unless the non-fixated light be very weak in intensity, it will leave an appreciable positive after-image streak. "The eye moves in a direction opposite to this latter light [non-fixated] and consequently the rapidity of its retinal displacement equals that of a light, perceived by a stationary eye, moving at a rate equal to the combined velocities of the two lights used in the pendulum test. Other things being equal, the length of the after-image streak varies directly with the rapidity of the retinal displacement. Thus a very pronounced length of the positive streak results in the test. This light, with its positive after-image, is viewed peripherally and hence is seen indistinctly and *en masse*; without conscious effort on the part of the observer, it appears as an elongated light with

<sup>1</sup> Carr, H., PSYCHOLOGICAL REVIEW, 1907, Vol. XIV., pp. 169-180.

<sup>2</sup> *Op. cit.*, p. 171.

<sup>3</sup> Aubert, H., 'Die Bewegungsempfindung,' *Pflüger's Archiv*, 1886, Bd. 39, S. 347-70; 1887, Bd. 40, S. 459-480.

no very decided contour, nor sharply discriminated parts.”<sup>1</sup> As the pendulum approaches the end of its swing, this elongated mass of light rapidly contracts in length at its rear end because both the eye and the non-fixated light come to rest so that the after-image streak is no longer generated, and the older (here the rear) portion of the after-image fades the sooner. “If the positive streak is six inches long when the pendulum is one inch from the end of its swing, and this streak has time to disappear while the pendulum is moving and returning over this final inch of its arc, it is evident that the total mass of light will have contracted at its rear end from six inches to one inch in length. These values are of course merely illustrative. . . . Consequently, the whole mass of light will appear to be moving on, after the pendulum has really stopped. The observed extra movement is thus a purely illusory one.”<sup>2</sup> It might be stated here that the author of the after-image theory does not assert this factor to be solely responsible for the illusion.<sup>3</sup>

The following conditions obtained in Carr’s experiments. The lower arm of the pendulum was 78 cm. in length, the upper arm, slightly shorter. The lower arm was made to swing through an arc of 160 cm. Two seconds were consumed in a forward and return movement (complete swing). The observer was 230 cm. away from the lights. Two very small incandescent lights of low intensity were used. The tests were performed at night in a dark room.

The following conditions obtain in the present writer’s experiments. Two miniature incandescent lights are attached to a counterbalanced pendulum. These lights are encased in wooden cups which have on their fronts circular openings of .5 inch diameter. The entire mechanism is placed behind a plate of ground-glass, the lights being brought very close to this glass. By careful regulation of the electric current and the use of small obstructive discs of milk-white celluloid, the intensity of the lights can be reduced to any degree. The lights are adjustable to any positions on the pendulum. Whenever it is desired to have a light at the axis, a third enclosed lamp is sus-

<sup>1</sup> Carr, *op. cit.*, pp. 171-172.

<sup>2</sup> *Ibid.*, p. 172.

<sup>3</sup> *Ibid.*, p. 178.

pended before the glass plate, and reduced to the required intensity. The best illusions are obtained by the writer when the lights are at short distances from the axis, *i. e.*, one inch to ten inches. The observer is stationed at a distance of seven feet from the lights. The tests are conducted in a dark room.

The pendular whiplash illusion is complex, its factors of illusion being: (1) the apparent unequal distances traversed by the two lights, (2) the apparent unequal speed of the two lights, and (3) the apparent movement of one light temporally longer than the opposite movement of the other.

I. *The Apparent Unequal Distances Traversed by the Two Lights.*—Dodge says: "If the distance of both from the axis was equal, both would move through equal distances in the same time. The one fixated however always appeared to move much less than the one seen peripherally. It was found that if the two were to appear to move through equal arcs, the pursued must actually move through about three times the arc of the unpursued. This of course could be accurately measured by the relative distances of the two points from the axis."<sup>1</sup> As regards his investigation of the above point, Carr has this to say: "My observers did not confirm these results as to the apparent lengths of movement. In fact, they gave judgments of equality of movement only when the two arcs were practically equal in length."<sup>2</sup>

My subjects gave judgments to the effect that the non-fixated light apparently moves a much greater distance than the one fixated. The difference of apparent distances traversed by these two lights is clearly shown in a modification of the experiment where a piece of cardboard, of sufficient size to allow the non-fixated light to be seen only at the very beginning and the end of its sweep, was placed over this light's path, this eliminating the after-image streak which might be thought responsible for the greater distance apparently moved through. The non-fixated light still appeared to travel much farther than the fixated. No exact measurements were made in the study of this factor (distance). It is undoubtedly significant that the non-fixated

<sup>1</sup> Dodge, R., *op. cit.*, pp. 13-14.

<sup>2</sup> Carr, *op. cit.*, p. 173.

light moves across the retina twice the angle that the eye-ball traverses in its socket in fixating the other moving light.

The illusion in the case of the modified experiment above was striking, though the positive after-image streak was eliminated as a factor in its determination.

For short arm lengths, there is a more marked difference between the apparent lengths of the arcs (described by the two lights) than for greater lengths, except where the opaque disc is used as already described.

II. *The Apparent Unequal Speed of the Two Lights.* — It seems natural that the non-fixated light should appear to move faster than the fixated, since it appears to move farther. It is the consensus of opinion of my subjects that such a difference is pronounced, yet no exact experimental data were secured to sustain such a view.

III. *The Apparent Movement of One Light Temporally Longer than the Opposite Movement of the Other.* — This factor is the whiplash illusion proper. The references and theories cited in the first part of this article, bear directly upon this factor.

There is an appreciable difference in the quality of the illusion for great, and very short lengths (of the pendulum arms). In case of a ten-inch length of the arms, the pendulum appears to bend at or near the axis, while in one- or two-inch lengths the pendulum appears rigid, the non-fixated light depending from the fixated as a point of support.

When very short lengths are used, so that the lights move slowly, a few of my subjects observed practically no motion of the fixated light.

It makes no difference in the illusion itself whether the lower or upper light be the one fixated. The illusion is equally appreciable for horizontal, vertical, and oblique oscillations. In the latter two cases however, greater difficulty of fixation is experienced. This is probably due to the comparative difficulty of vertical and oblique eye movements.

An interesting variation of the experiment is made by placing one of the two lights at the axis. When the moving light is fixated, an illusory motion of the stationary light is observed.



In order to establish a pendulum arm-length (within reasonable limits) at which the illusion is most pronounced, the contents of Table I. (which follows) were obtained. The several degrees of illusion are reported as Weak (W), Medium (M), and Strong (S). The absence of illusion is expressed by the letter N. Intermediate degrees are indicated by the plus and minus signs. For the purpose of numerical comparison, N, W-, W, W+, M-, M, M+, S-, S, S+, and S $\frac{1}{2}$ , are represented respectively by the arbitrary values 0, 1, 2, 3, 4, 5, 6, 7, 8, 9 and 10.

TABLE I.  
UPPER LIGHT FIXATED AND ATTENDED TO.

Name.	10"-10"			8"-8"			6"-6"			4"-4"			2"-2"			
H	S	S	S	S-	S-	S	S	M	S	M	M	M	S	S	S	→
R	S-	S-	S-	M+	S-	S-	S	S	S	S	S	S-	S+	S+	S+	→
F	S	S-	S	M-	M	M+	S-	S-	S	M+	S-	S-	S-	S	S+	←
R'	W	M	M	M	S	M	S	S	S	S+	S+	S+	S $\frac{1}{2}$	S $\frac{1}{2}$	S $\frac{1}{2}$	→
N	M+	M	M+	W-	M+	S	M+	S	M+	S	S	S	S+	S+	S+	→
M	M	M	M	M	M	M	S	S	S	M	M	M+	S	S+	S+	←
M'	S+	S+	S+	S-	S-	S-	M	S-	S-	N	M-	M-	N	W	N	←
N'	W	M	S	M	M	S	W	S	S+	M+	M	S-	M	M	M	←
Num. Equiv.	154			144			173			151			175			
Av. Equiv.	6.4+			6.0			7.2+			6.3+			7.3+			

NOTE:—(1) Figures at the top show the distances of the two lights from the axis of the pendulum. (2) Arrows indicate direction of procedure with the several subjects. (3) Each subject gave three judgments for each position of the lights. (4) Oscillations are horizontal; upper light fixated. These conditions prevail throughout the experimentation except for Table V when a third, or central, light is fixated.

From the, to be sure, somewhat irregular results of Table I., it would seem that the shorter lengths of the pendulum arms favor the illusion.

A modification of the regular experiment, made by placing a grating over the fixated light, gave very interesting results. In case the grating was placed over the fixated light while the pendulum was in motion and the illusion appreciable, a diminution of the illusion followed instantly. The grating adds the factor of image displacement on the retina to the usual perception of the motion (whatever this consists in) of the fixated light. This gives evidence in support of Carr's *limen* theory.

TABLE II.  
GRATING USED OVER FIXATED LIGHT.

Name.	'' 10-8			'' 10-7			'' 10-6			'' 10-5			'' 10-4			'' 10-3			'' 10-2			'' 10-1		
<i>H</i>	W	W	W	W-	W-	W-	W	M	W-	M	W	N	N	N	W-	W-	W-	N	N	S-	N	N	←	
<i>R</i>	S-	M+	S	M+	M+	M+	W	M+	W	M+	S-	W+	M-	M+	M-	M-	M-	M+	M+	S-	M+	M+	←	
<i>F</i>	S-	S-	S	M+	S-	S-	S	M+	M	M	M	M	S	M	S	S	S	W-	W-	W	W-	W-	←	
<i>N</i>	M	M+	M+	W	W	M-	W	M	W	W	M	M	M-	S-	S-	W	M-	M	M	M	M	S	←	
No. Eq.	69			51			49			47			47			40			40			41		
Av. Eq.	5.75			4.25			4.08+			3.91+			3.91+			3.33+			3.33+			3.41+		

Note: (1). Subjects *R'*, *M*, *M'* and *N'* were not available for this experiment.

By a comparison of Tables I. (or III.) and II. it is seen that the grating diminishes the illusion considerably. Out of ninety-six judgments in Table II. there are ten where the illusion was absent altogether.

Carr states (p. 177) that the illusion 'is conditioned by the direction of the attention.' In obtaining Table I. the importance of this statement was frequently realized. In preliminary tests seven out of eight subjects reported the illusion to be better when the attention was directed to the fixated light than when directed to the peripherally perceived light. It seems that when the fixated light is attended the non-fixated light is neglected, and so is permitted to execute its movement without in any way being checked, in other words, it is allowed to go free, whereas, in the event that the non-fixated light is attended, it is kept vividly in consciousness, and therefore we seem to *keep pace* with it and to perceive its motion as being perfectly normal. This led to the formation of Tables III. and IV., the results of the former being obtained by carefully attending the fixated light, those of the latter, by attending the non-fixated light.

The values of  $R$ 's judgments in Tables I. and III. are respectively, 143 and 132 points. This shows a situation contrary to his introspection that it is when the non-fixated and not the fixated light is attended that the illusion is greater. With the exception of  $R$ , it was reported by the observers in the experiments of Table IV. that whenever the illusion was perceived, it was distinctly felt at the time that the attention had wandered *back* from the non-fixated to the fixated light or to some intermediate point.

The decrease in the degree of the illusion (in Table III.), as indicated by the numerical equivalents, is very likely due to the distracting influence of the non-fixated light upon the attention as the lights become closer together. In other words, it appears that the attention, at short distances of the lights, is inclined to pass to the non-fixated light, thereby diminishing the value of the illusion.

We observe in Table IV. a general decrease toward the smaller distances. This appears reasonable, *for the non-fixated light can be more easily attended at the shorter distances.*

TABLE III.  
ATTENTION ON THE FIXATED LIGHT.

Name.	" " 10-8 "			" " 10-7 "			" " 10-6 "			" " 10-5 "			" " 10-4 "			" " 10-3 "			" " 10-2 "			" " 10-1 "		
H	M	M	M	M+	M	M	M	M	M	M+	M	M	M+	M	M	M	M	M	M+	M	W	W	W	↑W
R	S	S-	M	S-	M+	M	M+	M	M	M-	M	M	S-	S-	M	S-	M	M	M+	M-	M-	M-	M-	↑M
F	S+	S+	S	S+	S+	S	S+	S	S	S+	S	S	S-	S	M+	S-	M	M+	M+	W+	M-	M-	M-	↓M
N	S	S	S	S	S	S	S	S	S	M	M	M	M+	M	M	M+	S	M	M+	W	W	W	W	↓M
N'	M	M	M	M+	M	S	S-	S	S	W	W	W	M	M	M	S+	S+	S	M	W	M	M	M	↑M
No. Eq.	104			105		103				85			96			96			82					53
Av. Eq.	6.9+			7.0		6.9+				5.7+			6.4			6.4			5.4+					3.5+

TABLE IV.  
ATTENTION ON THE NON-FIXATED LIGHT.

Name.	10-8"		10-7"		10-6"		10-5"		10-4"		10-3"		10-2"		10-1"	
<i>H</i>	W-	N	N	N	N	N	N	N	N	N	N	N	N	N	N	N
<i>R</i>	S-	M+	M+	S-	M	M	M	M	M+	S-	M	M	M	M	M	M
<i>F</i>	W	N	W	N	W	W	W	W	M+	W-	W	W	N	N	N	N
<i>N</i>	W	N	W	N	W	W	W	N	N	W	N	N	N	N	N	N
<i>M'</i>	W	W	W	N	W	W	W	N	N	W	W	W	N	N	N	N
No. Eq.	37		32		33		20		25		23		25		20	
Av. Eq.	2.5+		2.1+		2.2		1.3+		1.7+		1.5+		1.7+		1.3	

Note: (1) The light *ten* inches distant from the axis is the one fixated during experimentation for Tables III. and IV. (2) *R'*, *M*, and *M'* were not available as subjects for Table III.; *R'*, *M*, and *N'* not available for Table IV.

In order further to test the influence of direction of the attention on the illusion, the following variation of the experiment was made. Three lights were used, two as before and the third made stationary at the axis of the pendulum. At all times this *central* light was to be fixated. After-image streaks were eliminated by reducing the intensity of the lights. The observer was given a position seven feet from the lights. His head was held stationary by clamps. A small light was thrown from the side on one eye, the other eye being covered.<sup>1</sup> A reading telescope was trained upon the eye so that the slightest motion could be detected. The two peripheral lights were each eight inches from the axis. The observer was instructed to direct his attention to *one* of the peripheral lights, his fixation being on the central light however.<sup>2</sup> Under these conditions the non-attended light regularly appears to move for a longer time than the attended light. The observer then reported each time that the illusion appeared. Fifteen judgments of illusion were taken for fixation of both lower and upper lights. The illusion is readily perceptible, but appears irregularly, depending upon the accuracy of the attention's direction. By the use

TABLE V.

THREE LIGHTS: CENTRAL BEING STATIONARY AND FIXATED.

Name.	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15
<i>H</i>	o	o	o	o	o	o	o	o	o	o	m	o	o	o	o
	o	o	o	o	o	o	o	o	o	o	o	o	o	o	o
<i>R</i>	o	o	o	o	m	o	o	o	o	o	o	m	o	o	o
	o	o	o	o	o	o	o	o	o	o	o	o	o	o	o
<i>F</i>	o	o	o	m	o	o	o	o	o	o	o	o	o	o	o
	o	o	m	o	o	o	o	o	o	o	o	m	o	m	o
<i>N</i>	o	o	o	m	o	o	m	o	o	o	o	o	o	o	m
	o	m	o	o	o	o	o	o	m	o	o	o	o	o	o

Note: (1) o = no eye-movement; m = eye-movement.

(2) For every judgment (whether o or m) there was an illusion.

(3) Each of the two pendulum lights 8 inches from the axis.

(4) Upper row of judgments for each subject is given for case where the upper light is attended, the illusion being of the lower light; the lower row of judgments, when the lower light is attended, the illusion being of the upper light.

(5) *R'*, *M*, *M'* and *N'* were not available as subjects for this experiment.

<sup>1</sup> In all previous experiments, both eyes were used.

<sup>2</sup> As to the possibility of such an adjustment, cf. Helmholtz's *Physiol. Optik*, 2te Auflage, 1896, S. 605.

of the telescope, the experimenter was able to note any eye-movement at or immediately preceding the instant of report. Out of one hundred and twenty judgments as seen in Table V., there were only twelve where the experimenter was at all uncertain as to the immobility of the eye.

This last experiment shows conclusively that the illusion is to a large degree influenced by the direction of the attention. The effect produced by the attended light is perceived *prior* to that of the non-attended light. When the attended light has reached the end of its swing, the non-attended light is of course neglected and slow to enter consciousness. When the non-attended light appears to be reaching the end of its swing, the attended is on its way back, having been transmitted to consciousness before the former. The apparent bending of the pendulum occurs at this time. *It is evident that the pendular whiplash illusion is in part a special case of the law of prior entry of attention.*<sup>1</sup>

Of the three factors of illusion, that of the whiplash *excursion* has received most attention at the hands of the investigators, but this in no wise should indicate that the factors of apparent distance and velocity of the lights are unimportant. Dodge has failed to give a theory accounting for the illusion. He uses it (the illusion) as an instrument in his attempt to prove that the pursuit movement is unable 'to subserve the perception of motion.' Carr offers the *limen* and *after-image* theories both separately and conjointly in explanation of the illusion. The *limen* theory, in addition to claims already advanced for it, is supported by Table II. of this article. There is no incompatibility between the *limen* theory and the *attention* factor. The positive after-image streak is possibly a factor of some importance, but it is well to remember that the illusion can be produced without the influence of this streak, as in the case where an opaque disc was placed over the path of the non-fixated light. The *after-image* theory is further weakened by the fact that when the after-image was eliminated (as above when a piece of cardboard was placed over the non-fixated light's path) the

<sup>1</sup> See Titchener, *The Psychology of Feeling and Attention*, p. 251, for the law of prior entry.

distance (apparent) traversed by the non-fixated light was *greater* than before. The results of Tables III., IV. and V. sustain the factor of *attention*.

From the foregoing investigation we seem bound to conclude that the pendular whiplash illusion is a complex phenomenon depending on one or all, according to the conditions, of the following factors: (1) the fading of after-image streaks at their older ends; (2) the lower threshold of movement perception by means of displacement of the retinal image, as compared with the threshold of movements as perceived by movements of the eyes; (3) the law of prior entry of attention.



## JUDGMENTS ON THE SEX OF HANDWRITING.

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In his book entitled *Les révélations de l'écriture d'après un contrôle scientifique*, Binet cites an investigation undertaken to decide whether it is possible to determine sex from handwriting. His conclusion is that within certain limits it is possible to do so. The question whether the sex-difference so discovered be due to psycho-physiological or social causes is left open.

It seemed to me worth while to repeat Binet's test in this country where, at least, the variation in writing induced by sex segregation in education would be minimized. A further motive for the test was furnished by the fact that the passing of such a simple judgment as 'man's writing' or 'woman's writing' seemed to offer particularly good material for a study of certain phases of judgment.

In the present test Binet's method was followed closely except in the particulars cited below. Briefly, his procedure was as follows. One hundred and eighty envelopes that had for the most part passed through the mails but from which all seals, headings and the like had been removed were submitted to two professional graphologists and to fifteen persons ignorant of the art of graphology. After a careful study of each superscription, the observer recorded his judgment as to the sex of the writer. Of the envelopes, eighty-nine had been addressed, either to Binet himself or to members of his family, by women; ninety-one had been addressed by men. As no selection had been made of envelopes to be used in the test the collection was held to be a representative one. Binet cites as a source of error the tendency to infer the sex of the writer from the sex of the person addressed.

This source of error was avoided in the investigation to be reported since all envelopes used were addressed to a woman. Two hundred envelopes were employed; all but four had

passed through the mails. One hundred had been addressed by women; one hundred by men. A considerable number of the two hundred persons whose writing appears in the series are known to have been educated wholly in co-educational schools. One hundred and fifty of the envelopes were collected by Dr. Grace R. Hebard as secretary of the board of trustees at the University of Wyoming. These envelopes were all superscribed to Dr. Hebard at the same address. They were numbered by her in the order in which they were received so as to insure a chance grouping within the series. The other fifty envelopes were addressed to me or to my sisters. The writers represented in the series include a considerable number of grade teachers and of college and university professors, many business men and women, a few lawyers, doctors and ministers, a few society women and women of leisure, a few literary people. On the whole, the collection is representative, although the teaching profession is unduly conspicuous.

These two hundred envelopes were submitted to thirteen persons, each of whom recorded his judgment as to the sex of the writer who had addressed each envelope. Whenever the writing on an envelope was recognized the judgment was thrown out. For the most part, however, the possibility of such recognition was slight since a very great number of the envelopes had been addressed by strangers to the community. None of the thirteen persons made any pretense to skill in reading handwriting; none was acquainted with the particular claims of graphology. Some of them had had, however, much more extensive opportunity to observe varied forms of penmanship than had others. In age, the observers varied from fifteen years to something over fifty. Eight of the observers had had some slight training in psychological experimentation; the others, none at all. Beside these judgments on the complete series of two hundred, five other persons gave judgments on a series of one hundred envelopes.

If in such a series of judgments, where there are only two possibilities to choose from, the number of right judgments exceeds fifty per hundred, it is evident that something other

than chance dictates the judgment. In every case in the present test, as in the French test likewise, the percentage made was sixty or over. The results, in fact, parallel closely those obtained by Binet. The percentages of right judgments given by ten of his observers (neither of the experts is included) ran as follows: 65.9, 66.4, 67, 68, 69, 69.3, 72.9, 73, 73, 73.<sup>1</sup> The percentages made in the present test were as follows: 60 (*P*), 60 (*C*), 61.5 (*B*), 64 (*Py*), 66 (*Ck*), 66.5 (*A*), 68 (*Bh*), 68.5 (*R*), 70 (*D*), 70.5 (*S*), 71.4 (*Sn*), 71.5 (*Cn*), 77.5 (*Ra*).

Binet concludes that it is possible, with a certain percentage of error, to determine sex from handwriting. Obviously in estimating this error it is permissible to take the best record made by any observer. In the French test this record was made by the professional graphologist, M. Crépieux-Jamin, with 78.8 per cent. of correct judgments.<sup>2</sup> Furthermore, it is of course possible, though hardly proper under the circumstances, to argue that greater natural facility and a better grounded science of graphology would reduce the percentage of error. For instance, *Ra*, who made the record in my test, was allowed to review her errors in the first hundred judgments and then asked to repeat the test for the second hundred. Her percentage of correct judgments rose to 80. In any case, however, the possibility of a considerable percentage of error makes the determination of sex from handwriting of problematic value where a high degree of certainty is required as in legal procedure.

It would be possible in certain ways to strengthen Binet's conservative conclusion as to the validity of judgments passed on so-called sex-differences in handwriting. In the present investigation, for example, I asked the observers to enter their judgments under the following rubrics: Confidence Great, Confidence Moderate, Confidence Slight, and Confidence None. If now the percentage of correct judgments be calculated from the number entered under the head of Confidence Great, we get the following figures in place of those given above: 66.3 (*B*), 67.4 (*C*), 70.5 (*D*), 70.9 (*Bh*), 72 (*A*), 72.1 (*R*), 72.7 (*Ck*), 80.1 (*Cn*),

<sup>1</sup>*Les révélations de l'écriture*, p. II.

<sup>2</sup>*Op. cit.*, p. 8. I am unable to get this percentage from Binet's figures. According to my reckoning the percentage should read 78.3.

81.1 (*Ra*), 82.4 (*Sn*), 82.9 (*Py*), 83.3 (*S*). The percentage of *P* is not included since he failed to use the exact rubrics named in the instructions. In every case given it is evident that the percentage of error has been considerably reduced. Within a certain range the revelation of sex appears to be quite evident. Binet shows also that M. Crépieux-Jamin's percentage of correct judgments rose when this percentage is reckoned on the basis of the judgments that he considered certain in distinction from those that he considered only probable.<sup>1</sup> Under conditions most favorable to the expert, according to Binet's report, the error is reduced to ten per hundred.

Another fact emphasizing the validity of the judgments under investigation is brought out by a further study of the results of the present test. The judgments of the thirteen observers on the two hundred envelopes were tabulated in such a way as to show the concordance of judgments on the part of the observers. The judgments of twenty-one of the two hundred envelopes showed complete unanimity; in the case of forty-two other envelopes there was only one dissenting judgment, so that in sixty-three cases there was practical unanimity of judgment on the part of the observers. Further, in all but fifty cases there was a strong preponderance of judgments (four or more) in favor of one or the other of the sexes. Such facts show conclusively that there exists, for the average person, a fairly definite conception of masculine and feminine penmanship.

The bearing of the evidence upon the fact of actual sex-differences is, however, blurred by what Binet calls the inversion of sex signs. In my test, of the sixty-three envelopes on which judgment was practically at one, nine were written by the sex opposite to that to which they were ascribed. In the 150 cases showing a clear preponderance of judgments in one direction or the other, twenty-seven such inversions occur. Binet gives reproductions of writing showing inversion of sex signs but fails to state the number of times such inversions occur — an unfor-

<sup>1</sup> *Op. cit.*, p. 9. I cannot determine from Binet's figures the percentage of correct judgments calculated from the number given with certainty. So far as I understand the figures the percentage is 82.3, closely approximating the best record made in my test.

fortunate omission which renders impossible a detailed comparison of his results with mine.

Again, examination of the present results shows something of which Binet makes no mention, namely, that these so-called inversions of sex signs occurred somewhat more frequently in the case of women than in the case of men; that is, the twenty-seven cases of notable inversion include seventeen cases of women's writing uniformly ascribed to men and only ten cases in which the reverse occurred. Furthermore, the records show a distinct preponderance of masculine judgments. Of the 2,592 judgments passed 1,351 were masculine, 1,241 feminine. The reason for this excess of masculine judgments is evident from the statements of certain observers relative to what they considered to be the signs of masculinity and femininity in handwriting. Obviously, originality is held to characterize the man's hand; conventionality, the woman's. Consequently, masculine handwriting is thought to show a more extensive range of variability than does the woman's. The plain evidence of such a constant drift in judgment is interesting in view of the fact that it follows the conclusion of certain scientists that men are much less conventional than women and show a greater range of variability. In the present case such a pressure of social judgment (if one may so express it) led to a constant error.

In this connection it is profitable to study the handwriting uniformly judged masculine or feminine. The typical feminine hand does appear to be colorless, conventional, neat and, usually, small. I should judge it unlike the feminine hand of the French test. In four cases alone does it exhibit individuality. Two of these four exceptions were graceful flowing hands; one an excessively rounded hand; one a backhand. Frequently this hand shows signs of unaccustomedness, that is, of being the handwriting of a person who had done no great amount of handwriting either because of youth or manner of life. Such unaccustomedness leads to conventionality in writing. This feminine hand is also in many of the cases the handwriting of grade or high-school teachers, whose profession emphasizes conventionality of writing. The typical man's hand is bold or

careless or experienced, above all, individual. It is written by professional men and women or by newspaper and literary folk.

Let us now study the twenty-seven cases showing inversion of sex characteristics. The ten masculine writers of feminine handwriting were as follows: one farm hand, one college student, two school teachers, two college professors, one business man, one lawyer, one minister, one left-handed superintendent of public instruction. The first four wrote the colorless conventional, somewhat unaccustomed hand already described; the second five wrote small neat flowing hands; the tenth penman wrote a labored backhand. At least two of these men have done no great amount of writing; four others are teachers.

The seventeen women whose writing was judged masculine were as follows: three county superintendents of public instruction, one public lecturer, one university secretary, three women who have held clerical positions, one college professor, four school teachers, two society women, two old ladies of over seventy years, one of whom had served for years as a librarian. Of these women, seven wrote either very bold or very rapid hands; six wrote very individual hands; the other four wrote careless sprawling hands. The point to be noticed is that at least half of the number have been accustomed to more than a usual amount of handwriting. This fact would apparently lead to a social rather than a psycho-physiological interpretation of sex-differences in handwriting. A reversed interpretation is of course possible since it is open to argue that original mental differences are basal and that the inversion of sex signs in handwriting points to an inversion in other respects. The trend of my results makes me exceedingly doubtful of such an interpretation. Granting the existence of sex mental differences based on profound psycho-physiological causes, one would scarcely expect to find an inversion of such differences eighteen times per hundred cases.

In this connection it is well to note the hand called by Binet ambiguous. It gives little evidence of sex. In the present test there were found twenty-one cases in which the judgments were practically evenly distributed between the two sexes. A study of the envelopes so grouped reveals in general two sorts of

handwriting. The one is, again, the manifestly inexperienced hand; the other a small supple hand, less bold than the so-called masculine hand, more individual than the so-called feminine hand. This latter hand appears to be, frequently, the handwriting of highly cultured persons.

Attention should also be called to age as influencing handwriting. Naturally the unaccustomed, hence conventional and labored, hand would more frequently characterize the younger penmen, solely on the ground of lack of experience. The writers producing in the present test the typical woman's hand probably averaged less in age than did those writing the man's hand, although there were striking exceptions. Crépieux-Jamin would ask that the handwriting of very old people be eliminated in a test on the revelation of sex in handwriting. It was evident that the very old ladies whose writing appeared in my series did not produce the typical woman's hand.

To sum up. From my analysis of my own results I conclude that it is possible to determine sex from handwriting in perhaps eighty cases out of a hundred. A detailed study of the cases showing inversions of judgments leads me to believe that the presence or absence of the so-called sex-signs is, in the case of any one writer, influenced largely (1) by the amount of writing done; (2) by age and consequently, to a certain extent, by practice; (3) by professional requirements such as shown by the conventional writing of grade teachers and the rapid hand of bookkeepers. As the majority of grade teachers are women and as women other than teachers as a general rule do less writing than men (professional men at least) women's writing on the average would be distinguishable from that of men.

On the face of it my results as presented are closely comparable to those obtained by Binet. Whether they are actually so could only be determined by a comparison of specimen with specimen. Particularly one would wish to compare the typical feminine and masculine hands of the two tests and above all the ambiguous hands and those showing inversion of sex signs. Such a detailed comparison would show how far the judgments passed are themselves of social origin.

My interpretation of my results would not, of course, be in

line with the claims of professional graphologists who inspect handwriting for the discovery of such signs of femininity as lack of energy, of clearness and of simplicity, and exhibition of vanity and self-consciousness.<sup>1</sup> On more general grounds than this one might expect a difference in the handwriting of the sexes, since previous investigations have given us reason for believing that men exceed women in motor ability.<sup>2</sup> The application of such a general principle to sex-differences in handwriting must, however, be turned over to those psychologists who are experimentally determining the range of variation in writing reactions.

In conclusion, a word relative to the types of judgment revealed by the experiment. The varying skill shown by the subjects of the test was most interesting. The proficiency displayed by one or two of them actually gives some color to the claim of those who believe in the absoluteness of the sex-revelation in handwriting and charge error to lack of insight. On the other hand, the fact that amateurs are able to approximate the record made by professionals who have given much time and thought to the art may be cited as showing that the errors of the latter may be charged not to lack of proficiency in interpretation but to the fact that there is an easily attained limit to the power of such interpretation. Not all handwriting does reveal sex.

Binet observed that, on the whole, the ability of his subjects to give correct judgments far outran their ability to ground those judgments in definitely assigned reasons. Certain of his subjects were much more deliberate than were others who satisfied themselves with a glance of the eye. Crépieux-Jamin, on the other hand, furnished Binet with an elaborate analysis of the signs of sex discerned by him in the case of each specimen passed upon. Binet was thus furnished with a mass of material to be correlated with correct and false judgments respectively. In this respect I was unable to duplicate his test. I made, however, some attempt to determine certain facts in regard to the judgments passed.

<sup>1</sup> J. Crépieux-Jamin, *L'écriture et le caractère*, p. 385 f.

<sup>2</sup> H. B. Thompson, *Psychological Norms in Men and Women*, Chap. II., p. 8 f.



It was desired to determine, first, whether the more deliberate or the more intuitive observer would give the higher percentage of correct judgments; secondly, to note individual differences in the matter of degree of confidence, and to determine the index of accuracy with assurance.

The judgments were entered under two all-inclusive headings. Judgments Immediate and Judgments Hesitant, the observers being instructed to enter a judgment under the second head whenever there was any pause for deliberation. Under each of these headings were placed the rubrics, Confidence Great, Confidence Moderate, Confidence Slight and Confidence None. A record was kept of the time taken by each observer in passing the judgments, this record to serve as an objective check on the division made by the observer into immediate and hesitant judgments. This check was somewhat crude, inasmuch as the time might be lengthened through mechanical slowness in the recording of judgments which had been quickly passed. In general, however, the subjects giving the greater number of immediate judgments performed the test in a shorter time than the other observers. *Sn*, who gave the greatest number of immediate judgments (89.7 per cent.), also carried out the experiment in the record time, sixty minutes. *A*, who gave the fewest immediate judgments (55.5 per cent.) required, with one exception, most time for the experiment, 115 minutes.

The immediate judgment represented a judgment on the writing as a whole, with little attention to details. The judgment is simply an identification of the writing as man's or woman's or a mediate classification on the basis of some rubric, as 'bold' hence 'masculine'.

Reference to the percentage of right judgments shows that *Sn* and *Ra*, who gave the highest percentages of immediate judgments, made respectively third and first place in accuracy of judgment on the whole series. *Ck*, *Bh*, *B* and *Py*, who ranked third, fourth, fifth and sixth with reference to immediacy of judgment, relative to accuracy, took, respectively, the ninth, seventh, eleventh and tenth positions. On the whole, therefore, a certain amount of hesitation appears to be of advantage.

Estimating the percentages of correct judgments for immediate and hesitant judgments, it is found, however, that *with one exception*, the immediate judgments give uniformly a higher percentage of correct judgments than do the hesitant. Partly, no doubt, this is due to the fact that it is the more difficult cases that induce hesitation. The subjects, however, shift their relative positions when ranked by reference to the accuracy of their immediate or hesitant judgments. Hesitation is, for certain observers, fatal, they are hopelessly confused by it. *D* improves in her judgment; *Ck*, who in other matters is quite capable of analysis and assisted thereby, appears in this test temperamentally adverse to hesitation. The hesitating judgments of *S* are very nearly as accurate as his immediate ones. Whether, then, it is better in this test to depend upon intuition or deliberation appears to be an individual matter.

Turning now to the certainty or confidence with which judgments were given one finds again a great range of variation. The percentages of judgments given with great confidence ran from 23.5 to 82.5 per cent., the no-confidence judgments from 0 to 21.8 per cent. Three of the observers recorded only three degrees of confidence; one confined herself to two only, namely, confidence great and confidence slight. A comparison of the percentages of correct judgments on the whole series with the percentage correct when confidence was great shows, as previously mentioned, a higher degree of accuracy when confidence is great. The individual differences are, again, notable. *D* gives a rise in accuracy of only .5 per cent.; *Py* gives an increase of 18.9 per cent.

A ranking of observers on the basis of their accuracy with assurance would be of prime importance if one wished to use such judgments in an objective way. Such a ranking would effect considerable shifting of relative position. *S*, who held fourth place in general accuracy with 70.5 per cent. of correct judgments, now moves to first place with a percentage of 83.3 correct judgments, an approximation of the professional record. *Py*, who held tenth place with 64 per cent. of correct judgments, now takes second place with 82.9 per cent. of correct judgments. Both *S* and *Py* are, however, exceedingly cautious,

giving, respectively, only 24.3 and 23.5 per cent. of confidence-great judgments. *Ra* and *Sn* still maintain their position among the first four most accurate observers, *Ra* with a percentage of 80.1 correct judgments on 58 per cent. of confidence-great judgments, *Sn* with 82.4 per cent. of correct judgments on 58.1 per cent. of confidence-great judgments. *Ra* and *Sn* are thus seen to be altogether the most valuable subjects since their index of accuracy is high and their assurance covers a considerable range.

There was some tendency for a high percentage of confidence-great judgments to be correlated with a high percentage of immediate judgments, although the range in the latter case is less than in that of the former. In every case there are but few hesitant judgments entered under the head of confidence-great; at most, only seven per cent. of the total number in the series.

On the whole, the individual differences thrown into relief by the test were interesting and striking. The rapidity with which some observers reached their decisions was in great contrast to the slowness of others. The variation in the assurance with which certain observers recorded their judgments contrasted with the lack of confidence exhibited by others. Noticeable, too, was the great variation in the value of the confident judgments of the different observers. One is not ready to conclude that the results of the present test would be typical of the same individual when dealing with other material, although the fact that no observer had had previous experience in passing such judgments put them, in this respect, all on the same level in the test under consideration. For all of them the test was a novel one. It may be said that the young woman giving the greatest number of great-confidence judgments (82.5 per cent.) bears a reputation in the college community of being confident and decided in her opinions; the one giving the lowest percentage of confidence-great judgments (23.5 per cent.) is peculiarly self-distrustful and diffident.

Two methods of basing the judgment were described. The first method was analogical, based on vague popular inductions concerning masculine and feminine characteristics. The second

method involved a reference to types or classes of handwriting, a more or less clear-cut classification dependent upon one's experience. The more extensive this experience and the more interest had been directed to the inspection of handwriting, the more this method prevailed. A distinct reference to one or more similar hands was often remarked. For some, this reference seemed to be to a generic rather than to a specific image. One subject who carefully reviewed her judgments with the envelopes and the list of penmen before her stated that a frequent error had been the reference of a whole class to the wrong head. Her classificatory type was clear-cut but its label 'masculine' was incorrect. How far these methods of judgment could be described as contributory, respectively, to logical or psychological certainty is not evident. Judgments given with confidence occurred in both cases. My records are not complete enough to show whether individual variations occurred in the degree of confidence accompanying these two sorts of judgment.

## THE CHANGE OF HEART RATE WITH ATTENTION.<sup>1</sup>

BY M. LEROY BILLINGS, A.B., AND JOHN F. SHEPARD, PH.D.,

Writers on the relation of mental and nervous conditions and organic changes, such as those in the circulation, have usually treated the various organic reactions as though they were relatively independent of each other. Studies of the influence of stimuli upon the heart rate have been made without a sufficient consideration of the breathing and Traube-Hering waves, and especially of the effect of change in the depth and rate of breathing. In an earlier work<sup>2</sup> by one of the writers it is suggested that the temporary slowing of the pulse which one often gets with sensory attention is due to the decreased rate and amplitude of the breath under these conditions. While the results discussed there suggested such an explanation, the point needed further study. This paper is a report of the results obtained from experiments designed to show more definitely the factors concerned.

To get the pulse in these experiments we used the Hallion-Compte plethysmograph. It was connected with a piston recorder writing upon a kymograph. For the breath we used Sumner's pneumographs, one around the chest and one around the abdomen. These were connected to Marey tambours. The kymograph used was the Zimmermann with a long extension paper about eight feet in length. Writing upon this same kymograph, in a vertical line with the piston needle and the tambours, were an electric marker to indicate when the stimulus was given (in referring to this line hereafter we shall call it the reference line), and another which marked the time in fiftieths of a second. This latter was run by an electric current interrupted by a tuning fork. In these experiments the drum turned rapidly

<sup>1</sup> From the Psychological Laboratory of the University of Michigan.

<sup>2</sup> Shepard, 'Organic Changes and Feeling,' *The American Journal of Psychology*, 1906, p. 554.

enough so that the time could be counted very accurately in two-hundred-fiftieths of a second. The recording apparatus just described was placed in one room and the subject in another so that the noise of the operator and the running of the kymograph did not disturb him.

The subjects for these experiments besides one of the writers, Mr. Billings, were Mr. Dockeray, a graduate student and assistant in psychology, and Mr. Carpenter, an undergraduate student who was also taking special work in psychology. We wish to acknowledge our indebtedness for their kind assistance.

A number of records were taken simply to show the effect of retarded breathing. A normal curve was taken for some seconds, and then the subject was given a signal to consciously restrict his breathing. Likewise the effect of increased breathing was studied. Then different types of attention were investigated; first, attention to some central process such as recalling the forms of a French verb; secondly, attention to a visual stimulus such as reading at a distance; and thirdly, attention to an auditory stimulus, trying to detect a faint sound. Immediately after a record for a sensory attention was taken, another was taken in which the subject was asked to reproduce the changes of breathing of the attention record as nearly as possible. In the first tests he was merely asked to begin at a signal and to repeat the method of breathing of the attention experiment as nearly as he could from memory; but, in the latter experiments we set up another kymograph (number two) directly in front of the subject with a pneumograph and tambour as before. Then, while the subject was working, we were getting a curve of his respiration on this kymograph as well as on the main one. Now, when he was to reproduce a record we raised the kymograph (number two) so that the point of the recorder came just under the original tracing. By this method, when the apparatus was going, the subject could reproduce exactly the original record. In the interpretation of these latter reproductions, we must remember that we have a sensory stimulus in each one of them as well as in the original record. So the only difference between the two is in the normal. In the original record we have a normal curve followed by a curve influenced

by a sensory stimulus, while in the reproduction we have at the beginning a curve made by a normal breathing plus a sensory stimulus followed by an identical reproduction of the original.

After taking thirty-seven records we worked them up carefully as follows: The beginning of every pulse beat was marked with a sharp needle under a magnifying glass, and from each one of these points was erected a perpendicular to the time line. Then, the length of each pulse was counted off between these perpendiculars to two-hundred-fiftieths of a second. The errors in this counting are very slight if any. And in any case an error of one or two units would be of no importance.

For plotting the breathing curve we drew a line parallel to the reference line which represents the position that the tambour needle would have held had it not been disturbed by the movements of the breathing—in other words, the level line of the tambour-needle. This was used as a new base-line. From this the height of the breath at the beginning of each pulse and at the crest and trough of each breath was measured in millimeters and plotted.

The plotted records do not always show directly how accurately the breathing was reproduced, for since the plotted rate is measured in pulse-beats, and these are faster in one case than in the other for different reasons, the apparent length of the breath varies more than the original curve would show.

After having worked up all these records the results were plotted on coördinate paper. The *X* axis, or the standard line for the heart rate, was given the value of thirty-five, forty, or forty-five fiftieths of a second (represented as P35, P40 or P45 at the left end of the curve), and each unit of space above or below was given the value of one fiftieth of a second. Thus a slowed heart rate, *e. g.*, forty-eight fiftieths of a second, would be positive and so plotted above the *X* axis, and any value below that of the standard line would be negative and would be plotted below. So as the heart rate increases we have a decrease in the pulse length and a fall in the curve on the coördinate paper, and *vice versa* for a decrease in the heart rate. Each unit along the *X* axis marks off one heart beat. The dotted line perpendicular to the standard line indicates when the signal or the stimulus

was given. The breathing record was plotted along with the heart rate curve on the same paper and in a similar manner, excepting that different values were given to the spaces and to the base line (the  $X$  axis). The latter was given the value of 0 and each space vertical to it the value of one millimeter. The spaces along the  $X$  axis indicate the pulse beat as before. Thus the depth of the inspiration is plotted below the standard line and the height of expiration above it. The dotted curve indicates the heart rate curve and the full line the breathing curve. In the discussion we shall mean by the *pulse wave* the long wave in the heart rate including several breaths and Traube-Hering in character. The significance of the *breathing wave* is obvious.

The following are the results of the records as they were plotted :

#### RETARDED BREATHING.

No. 1B. No stimulus was given, the subject simply attended to his breathing, making it slower after the signal. He reported a disagreeable feeling from the lack of breath. The result was that we got a gradual decrease in the heart rate beginning at the signal and lasting throughout, and no change in the breathing or pulse waves.

Nos. 3B, 21D, and 37B. The results in these three were all alike. We got a very decided decrease in heart rate, an increased pulse wave, and an increased breathing wave. See plate No. 21D. The introspections in these experiments were the same as in No. 1B.

No. 5B. In this there was a decreased heart rate, an increased breathing wave, but no change in the pulse wave. Besides being retarded the breathing was increased in amplitude.

No. 7B. This was the only record that was taken with the retarded breathing that did not show a decided decrease in the heart rate; there was practically no change in this one, although it showed an increase in the breathing and pulse waves. The deviation of this record from the rest may be accounted for by the increase in amplitude.

Nos. 17D and 19D. These gave only decreased heart rate with practically no change in the breathing or pulse waves.



## INCREASED BREATHING.

Nos. 9B and 22D. There were no stimuli in these experiments. The subjects simply increased the rate of their breathing, and in No. 22D the amplitude was increased. The subjects both reported an agreeable feeling seeming to make up for the lack of breath in the previous experiments. In No. 9B the subject felt like laughing during the whole experiment, and in 22D the subject felt like taking a deep breath each time. The result was that we got a marked increase in the heart rate, a decrease in the breathing wave, and a total elimination of the pulse wave. See plate No. 22D.

## SENSORY STIMULI (AUDITORY).

No. 2B. The stimulus used in this experiment was listening to a very faint sound. A watch was placed so that the ticks were just audible. The breath was held so that each one was from three to four times as long as a normal breath, and the amplitude was slightly increased. The subject reported a feeling of lack of breath and some strain. The sound was very weak and came and went. The results were a slight decrease in the heart rate and an increase in the breathing and pulse waves. These changes began immediately following the signal. See plate No. 2B.

No. 3B. This was a reproduction of No. 2B. The subject was asked to reproduce the breathing as experienced in No. 2B as nearly as he could from memory. His breath was retarded and gave a fairly good reproduction for this method. It shows in No. 2B how the attention holds down the heart rate curve while in No. 3B without so much attention and with practically the same breathing the heart rate curve rises. See plates 2B and 3B. The subject reported a similar feeling in the two experiments.

No. 6B. The stimulus in this experiment was auditory, the same as in No. 2B. The breathing was very much retarded, especially at the first where it was held for several seconds and the amplitude was somewhat increased. The subject reported a lack of air, a disagreeable feeling from strain about the neck and head, and a tendency to move toward the sound. It resulted

that we had a very decided decrease in the heart rate from the very beginning, gradually becoming less as the subject began to breathe more, but at no time coming back to normal. Both the pulse and breathing waves were increased. The subject moved and took a deep breath during the normal.

No. 7B. In this experiment the subject tried to reproduce the breathing and the physiological conditions as experienced in No. 6B, but it was not a very good reproduction. The breath was simply retarded throughout. The results were that we got an increase in the pulse and breathing waves, but practically no change in the heart rate. The introspections were the same as were given in No. 6B.

No. 12C. The stimulus in this experiment was the same as in No. 2B. Immediately following the signal was a slowing of the breathing with the amplitude remaining about the same. The subject reported that he held his breath with expiration rather than with inspiration. He would lose the sound and then hold his breath until he caught it again. It was disagreeable, but not from the lack of breath. The results were a slight gradual increase in the heart rate from the beginning lasting throughout the first half of the record. In the last half of the record we got a partial return to normal. There was a gradual increase in the breathing and pulse waves throughout.

No. 18D. The stimulus in this experiment was the same as used in No. 2B. Immediately following the signal for about one third of the record, was a slight increase in the breathing rate and a decrease in this amplitude. This was accompanied by an increase in the pulse rate at first followed by a normal rate. The breathing then came back to normal and we had a very rapid increase in the heart rate, not much of any change in the breathing wave, but a total elimination of the pulse wave. After the subject had become quiet and attentive the sound was too loud, so instead of attending merely to the sound, he attended to the change of intensity of the sound. The subject felt a slight strain and lack of breath at first in expectation. He was also conscious of his chest breathing.

No. 19D. This was a reproduction of No. 18D though not a very good one. The breathing was retarded too much and the

amplitude decreased. The results were a decreased heart rate, a decreased pulse wave, but no change in the breathing wave. The subject felt a strain and expectation and lack of breath before starting.

Nos. 32B and 33B. The stimuli used in these two experiments were also the same as used in No. 2B. No. 33B is a continuation of No. 32B with a period of about two minutes between. The work of attending to the watch was kept up during this interval. This time was taken between the records to give the operator time to change the papers on the kymograph. The rate of the pulse did not fall as much as might be expected between the two records, but this can be accounted for by the fact that when the kymograph was not running it was not so difficult to hear the sound. Immediately following the signal we got a decided retardation of the breathing with a decreased amplitude. Corresponding to this we had a decrease in the heart rate for about four breaths; then we got a gradual increase throughout the two records with the exception of the last two breaths which were decidedly retarded and decreased in amplitude, and here we got again a slight decrease in the heart rate. There was a very small breathing wave throughout these two experiments which suffered little change. There was practically no pulse wave. In these two records it will be noticed that the heart rate was at first slowed by the decreased breathing, but as the attention became intense the heart rate was increased in spite of the tendency of the decreased breathing to make it go slower.

Nos. 34B and 35B. These were reproductions of Nos. 32B and 33B. They were very good reproductions with the exception that at the end of 34B the breathing was increased a little when it should have been retarded all the way through. The result was that we got a very much decreased heart rate from the beginning, with a gradual increase corresponding to the increased breathing rate. Beginning with No 35B we got a gradual decrease in the heart rate throughout with increased breathing and pulse waves.

#### SENSORY STIMULI (VISUAL).

No. 4B. The stimulus used in this experiment was reading at a distance. The subject read a German article at a distance

of about six feet, which made it quite a difficult task. Directly following the signal there was very little change if any in the breathing. This was accompanied by an increase in the heart rate, an increase in the breathing wave, and a decrease in the pulse wave. Then in the last half of the record there came a slight decrease in the amplitude of the breath. Accompanying this we got a decrease in the heart rate until it came back to normal, and stayed there, or a little above, throughout the rest of the experiment. There was a slight increase in the pulse wave. The subject reported some lack of breath, a tendency to move toward the stimulus, eye strain, and some muscular strain of the neck and body. The experiment was more or less disagreeable.

No. 16D. The stimulus used in this experiment was like that used in No. 4B, only made a little more difficult. The subject reported that after about the first quarter of the experiment there was a feeling of lack of breath which steadily grew to the end. Two or three times when he started to breathe he lost his place. He was aware of a slight strain. The breathing rate was increased but a very little but the amplitude was quite perceptibly decreased; this remained uniform throughout the rest of the experiment. The heart rate was increased with surprising rapidity from the giving of the signal. It increased from forty-five fiftieths of a second to thirty-five fiftieths. Toward the end of the experiment the heart rate decreased for just three breaths, rising even to normal rate but quickly dropping back again to its rapid speed. We cannot account for this decreased heart rate for so short a time. There must have been some other change either physiological or mental that the subject did not give in his introspection. This was the only case of the kind. The breathing wave was decreased and the pulse wave was almost eliminated.

No. 17D. This was a reproduction of No. 16D, but the pneumograph was cut off in the first part of the experiment so that we did not get a normal curve. It would be hardly fair to judge this reproduction without the normal portion. As far as the last part was concerned it was well reproduced. The heart rate gradually decreased from the signal to the end. There

was no change in the breathing wave but a decrease in the pulse wave. There was strain and expectation before starting.

Nos. 24B and 25B. The stimuli used in these two experiments were counting time marks that were very close together. The breathing was very nearly normal throughout record No. 24B with the exception of three breaths immediately following the signal, which were retarded. For about one third of 25B, which was a continuation of 24B the breathing was slightly increased in rate and decreased in amplitude. Accompanying this there was a decided increase in the heart rate, no change in the breathing wave, and an elimination of the pulse wave, excepting that which accompanied the three breaths following the signal. Here the heart rate was decreased, and the breathing and pulse waves were increased. In the last two thirds of record No. 25B the breathing was very much retarded, accompanied by a decrease in the heart rate to the normal, which remained so throughout. The breathing and pulse waves were slightly increased compared with the first part. See plate No. 24B.

Nos. 26B and 27B. These were reproductions of 24B and 25B; they were very good. Following the signal to the end we got an almost identical reproduction with the exception of about four breaths which are indicated by Xs on the plate. In the reproduction of the normal the heart rate was greatly increased thus showing the effect of attention, or a sensory stimulus, upon the heart rate. See plates Nos. 24B and 26B.

Between records 24B and 25B while the operator was changing the paper on the main kymograph there was a period of two minutes, but the work was continuous and kymograph No. 2 was kept running. In the reproduction the same time was taken in changing the papers, and the subjects kept reproducing what was done during the similar interval in the first record. So the reproduction was an accurate one all the way through.

Nos. 28B and 29B. The stimuli in these two experiments were the same as used in Nos. 24B and 25B. The breathing rate after the signal was very irregular, but in all cases except those of two or three breaths, was retarded. In five instances the breath was held for a time equal to three normal breaths.

The result was that we got an increased heart rate which remained throughout the experiment with the exception of the times when the breath was held; at these intervals the heart rate curve went up to normal but soon fell again. There was very little change in the breathing and pulse waves.

Nos. 31B and 32B. These were the reproductions of Nos. 28B and 29B. The results were the same as those obtained in Nos. 26B and 27B only not so marked.

#### MENTAL STIMULI (PROBLEMS).

No. 8B. In this experiment the subject was given a mental problem. He was told to conjugate a French verb. The breath was both retarded and increased in amplitude. Accompanying this, the pulse wave was decreased, the breathing wave suffered no change, and there was scarcely any change in the heart rate; it stayed practically normal. The subject reported a slight feeling of strain, and some expectation of the signal before starting, but did not notice any change in his breathing.

No. 15C. The problem in this experiment was to recall in chronological order the events of the Civil War. There was no change in the respiration but we got a very rapid increase in the heart rate from the beginning lasting to the end. There was a gradual decrease in the pulse wave until it was entirely eliminated. The breathing wave was not changed. See plate No. 15C. The subject reported that the effort was uniform throughout, there was some strain, and it was somewhat disagreeable in that he could not think of a couple of events. He did not notice any change in the respiration.

No. 20D. The problem in this experiment was to say the alphabet backwards. The breathing rate was slightly increased and the amplitude of the breath was slightly decreased. Accompanying this was a very marked increase in the heart rate, a rapid decrease in the pulse wave, and but little change in the breathing wave. The subject reported that he visualized the alphabet from a sheet of paper on which he had been working with the alphabet on it, but it was difficult to do this all the way through. There was some feeling of strain and lack of breath before starting.

No. 23B. The problem in this experiment was to begin with

nineteen and add rapidly by seventeens. The breathing was nearly normal, only retarded for a couple of times and those only for a couple of breaths. There was a gradual increase in the heart rate throughout with very little change in the breathing wave. The pulse wave was very slightly increased. The subject took a long breath just before starting. It was difficult to begin the problem but grew easier toward the end. Just at the end he forgot his last number, and then had to work extra hard to recall it again. See plate No. 23B.

No. 36B. The problem in this experiment was to begin with seventeen and add by nineteens. The breathing rate was normal and the amplitude of the breath was increased. Directly following the signal we got a quick increase in the heart rate followed by a slight return which stayed uniform throughout the rest of the experiment but at no time came back to normal. The breathing and pulse waves were little affected. The subject was startled at the signal to begin.

#### SUMMARY.

We may summarize the results as follows: With close visual attention the breathing is uniformly decreased in amplitude. In rate it is sometimes increased, sometimes decreased, and sometimes not changed at all. With auditory attention it is nearly always decreased in rate, but changed irregularly in amplitude. The breathing in the kind of central attention that we used is very little changed. These changes are probably adaptive; they remove a source of disturbance. Deep breathing, with its accompanying movements, would interfere with looking; rapid breathing interferes more with the listening.

With the effort of attention the strain tends to increase the heart rate. Increased breathing, either in rate or amplitude, tends to increase the heart rate. Restricted breathing, either in rate or amplitude, tends to decrease the heart rate. For the latter reason one often finds a decreased heart rate with sensory attention, particularly at the first. With central attention the heart rate is regularly increased.<sup>1</sup>

<sup>1</sup>This physiological effect of breathing may account for the divergent reports in the literature. So some writers might find a slowed pulse at times with

With restricted breathing and decreased heart rate the pulse wave is markedly increased; with increased breathing and increased heart rate we get a decreased pulse wave. The heart rate change seems to be the more prominent factor with the pulse wave. It tends to decrease the wave when it itself is increased in attention in spite of the retarded breathing, but the lack of breath probably has some influence.

In studying the expression of the emotions it would be desirable to know more thoroughly the relation of the breathing and circulation changes in sorrow, joy, etc. An attempt will be made to do this later.

strain. Stevens concludes that the psychophysical processes of sensation are different with visual, auditory, and tactual stimuli. His comparatively rough method of taking results makes an exact investigation impossible; but a study of his published records suggests that, as far as any conclusion can be drawn from them, the differences are due to these changes in breathing.



# THE PSYCHOLOGICAL REVIEW.

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## THE METHOD OF CONSTANT STIMULI AND ITS GENERALIZATIONS.<sup>1</sup>

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We introduced in several previous publications<sup>2</sup> the notion of the probability of a judgment, which is the fundamental notion in the analysis of psychophysical measurement methods. The judgments of a subject who compares two stimuli under well-defined and constant conditions, have the formal and material character of those chance events which are spoken of in the calculus of probabilities. These probabilities are *a priori* entirely unknown and must be determined by observation. Experience shows that if a standard stimulus of given intensity is compared under constant conditions with stimuli of varying intensities, the probabilities of the different judgments vary in a certain way with the intensity of the comparison stimulus. Let the subject be required to express his judgment in either one of the terms smaller, equal or greater. The probability of the judgment greater increases and that of the judgment smaller decreases with increasing intensity of the comparison stimulus, whereas the probability of an equality judgment first increases and then decreases after having reached a certain maximum.

<sup>1</sup> An abstract of this paper, but without the tables, was presented at the spring meeting of the experimental psychologists at Harvard in 1908. A full presentation of this topic with all the necessary demonstrations was given in the treatise on 'Die psychophysischen Massmethoden als Grundlagen empirischer Messungen' in Vols. 15 and 16 of the *Archiv f. d. ges. Psychologie*.

<sup>2</sup> *The Application of Statistical Methods to the Problems of Psychophysics*, Philadelphia, 1908; 'On the Method of Just Perceptible Differences,' *PSYCHOLOGICAL REVIEW*, 1907, Vol. 14, pp. 244-253; 'Die psychophysischen Massmethoden als Grundlagen empirischer Messungen,' *Archiv f. d. ges. Psychologie*, Vols. 15 and 16; also in the report on 'Die Psychologie in Amerika,' *Archiv f. d. ges. Psychologie*, 1908, Vol. 11, 'Literaturbericht,' pp. 141-143.

This suggests the view that the probabilities of these judgments are functions (in the mathematical sense of the term) of the intensity of the comparison stimulus. A mathematical expression which gives the probability of a judgment as function of the comparison stimulus, is called the psychometric function of this judgment. These expressions are, of course, different for different intensities of the standard stimulus. It is not necessary but perhaps advisable to insist on the fact that such an expression refers only to well-defined experimental conditions, because it is not possible to assign definite values to these probabilities unless one refers to definite physical and psychophysical conditions under which the observations are made. The term psychometric function was chosen in imitation of the term biometric function, which is commonly in use for mathematical expressions which give the so-called probability of dying as function of age. If the psychometric functions of all the judgments are known, one is able to predict the outcome of any set of experiments.

For the practical application of this notion not only the form of these functions but also the values of all their constants must be known or data must be at hand by which they can be determined. Our choice of the expressions which may represent the probabilities of the different judgments as functions of the intensity of the comparison stimulus must be guided by the following consideration: Experience shows that the comparison of a standard with much greater intensities results always, or nearly always, in the judgment greater, whereas the judgment smaller is given exclusively, or almost exclusively, on the comparison with very small intensities. None of the functions, furthermore, must assume values greater than one and smaller than zero, because they represent mathematical probabilities. From this it follows that the psychometric functions of the greater and of the smaller judgments approach the values zero and one asymptotically, the psychometric function of the smaller judgments decreasing and that of the greater judgments increasing with growing intensities of the comparison stimulus. The psychometric function of the equality cases has a certain maximum, on both sides of which it falls off steadily and approaches

the value zero asymptotically. Add to this the condition that for any given intensity of the comparison stimulus the sum of all the psychometric functions must be equal to one. These conditions, however, are not sufficient to determine the nature of these functions and one must form a hypothesis about them, which may be based either on experience or on theoretical considerations. The number of psychometric functions in any set of experiments is equal to the number of judgments admitted. There are always two functions, the psychometric functions of which are similar to those of the smaller and greater judgments; judgments of this type are called extreme judgments. Judgments the psychometric function of which is similar in its course to that of the equality judgments may be called middle judgments. The number of middle judgments is always uneven; until now one has not found it necessary to go beyond the number of three middle judgments.

There is no special difficulty in devising algebraic expressions which may serve as hypotheses on the psychometric functions. A criterion of the value of different hypotheses can consist only in the greater or smaller agreement with experience, which is measured by the sum of the squares of the deviations of the observed from the calculated values. A mathematical expression, which may serve as a hypothesis on these functions, depends on a number of constants which must be determined by observation. These observations consist of empirical determinations of unknown probabilities and are, as such, subjected to errors, which prevent us from determining the exact values of the constants. This must cause differences between the true and the calculated values even if our hypothesis on the nature of the psychometric functions is correct. We will speak in this case of a lack of agreement between theory and observation on account of errors of observation. But it also is possible that our hypothesis on the psychometric functions is incorrect and in this case there must be a lack of agreement between theory and observation, even if all the constants were absolutely exact. Errors which are due to an incorrect hypothesis on the psychometric functions may be called errors of theory. It is not easy to decide whether in a certain case we have to deal with errors

of observation or with an error of theory, because, errors of observation being inevitable, errors of theory are always intermingled with them. Usually, however, this problem will be put in this way: that it is required to decide which one of a certain group of functions is best suited to represent a given experimental material. The errors of observation in this case remain the same for the different hypotheses and the sums of the squares of the deviations of the calculated from the observed values indicate the greater or smaller agreement of a hypothesis with experience.

One may try to get along without making a definitive hypothesis on the nature of the psychometric functions and, starting from the theorems that every function may be represented by a power series, one may determine from the data of observation as many coefficients as possible. If  $n$  comparison stimuli were used in the experiments one may determine  $n$  constants, by which all the terms up to that of degree  $(n - 1)$  are determined. This representation of the data of observation were absolutely exact, if the psychometric functions could be represented by an algebraic equation of degree  $(n - 1)$ , but even if this is not the case this method is distinguished in so far as it requires the smallest amount of theoretical additions to make a mathematical treatment of the data possible. The degree of the equations which represent the psychometric functions depends on the number of observations and is different for different sets of experiments, unless it happens that the same number of comparison stimuli had been used. In this sense one may say that the representation of the data by means of an algebraic equation does not involve a definitive hypothesis on the psychometric functions at all. It is easy to see, however, that this method can not possibly give a definitive result. The psychometric functions represent mathematical probabilities and are, as such, restricted to the interval from zero to one, whereas an algebraic function exceeds every limit for sufficiently large values of the independent variable. Lagrange's formula of interpolation and Newton's method of differences are the most convenient ways of treating the data according to this hypothesis and of determining new values of the psychometric functions

without actually setting them up. We will illustrate this method by working out a set of results which has served for the illustration of the method of just perceptible differences.<sup>1</sup>

TABLE I.

RELATIVE FREQUENCIES OF THE JUDGMENTS GREATER, SMALLER  
AND EQUAL.

Comparison Stimulus.	Equal.	Greater.	Smaller.
84	0.0444	0.0222	0.9333
88	0.1133	0.0244	0.8622
92	0.1889	0.1111	0.7000
96	0.2578	0.2933	0.4489
100	0.2400	0.5289	0.2311
104	0.0889	0.8156	0.0956
108	0.0800	0.9044	0.0156

Let us suppose we made observations with  $n$  comparison stimuli which we call  $x_1, x_2, \dots x_n$  and that these comparison stimuli gave to a certain judgment the probabilities  $a_1, a_2, \dots a_n$ . Lagrange's formula then has the form

$$\begin{aligned}
 F(x) = & \frac{(x - x_2)(x - x_3) \dots (x - x_n)}{(x_1 - x_2)(x_1 - x_3) \dots (x_1 - x_n)} a_1 \\
 & + \frac{(x - x_1)(x - x_3) \dots (x - x_n)}{(x_2 - x_1)(x_2 - x_3) \dots (x_2 - x_n)} a_2 \\
 & + \dots + \frac{(x - x_1)(x - x_2) \dots (x - x_{n-1})}{(x_n - x_1)(x_n - x_2) \dots (x_n - x_{n-1})} a_n.
 \end{aligned}$$

The actual setting up of the equation is laborious but not necessary for interpolating new values of the function. It is a matter of course that one must arrange the computation systematically whenever one has to treat an extended experimental material. The circumstance that the sum of all psychometric functions for any intensity of the comparison stimulus must be equal to one, shortens the work considerably. If three judgments were admitted it is sufficient to calculate by Lagrange's formula the probabilities for two judgments only, that of the third being found by subtracting the sum of the other

<sup>1</sup> PSYCHOLOGICAL REVIEW, Vol. 14, p. 249, 251. These results are taken from subject II, in the experiments on lifted weights, cf., 'The Application of Statistical Methods to the Problems of Psychophysics,' pp. 178, 217, and *Arch. f. d. ges. Psychologie*, Vol. 15, p. 287.

two from the unit. This is the way in which the interpolated values in Table II. were found. From the data of such a table a diagram may be constructed which shows graphically how the probabilities of the judgments vary with the intensity of the comparison stimulus. Such a diagram is shown in Chart I. The intensities of the comparison stimulus are represented on the abscissa and the corresponding values of the psychometric functions on the ordinate. It is necessary to draw the two axes on different scales; in our drawing the unit of measurement of the  $y$ -axis is ten times as large as that of the  $x$ -axis.

TABLE II.  
COMPARE CHART I.

Comparison Stimulus.	Equal.	Smaller.	Greater.
84	0.0444	0.9333	0.0222
85	0.0614	0.9101	0.0285
86	0.0786	0.8956	0.0258
87	0.0959	0.8814	0.0227
88	0.1133	0.8623	0.0244
89	0.1310	0.8353	0.0337
90	0.1497	0.7987	0.0516
91	0.1690	0.7533	0.0777
92	0.1889	0.7000	0.1111
93	0.2088	0.6407	0.1505
94	0.2279	0.5775	0.1946
95	0.2447	0.5129	0.2424
96	0.2578	0.4489	0.2933
97	0.2654	0.3875	0.3471
98	0.2659	0.3301	0.4040
99	0.2578	0.2779	0.4643
100	0.2400	0.2311	0.5289
101	0.2125	0.1900	0.5975
102	0.1761	0.1541	0.6698
103	0.1334	0.1230	0.7436
104	0.0889	0.0955	0.8156
105	0.0495	0.0710	0.8795
106	0.0251	0.0494	0.9255
107	0.0294	0.0303	0.9403
108	0.0800	0.0156	0.9044

There are two difficulties in treating the data by Lagrange's formula of interpolation. It happens in many cases that values are obtained which are greater than one or smaller than zero. The occurrence of these impossible values can have symptomatic value only. Their presence is easily seen in a graphic representation of the functions, because in these places the

curves rise above a line drawn parallel to the  $x$ -axis at the distance unity, or fall beneath the abscissa. The second difficulty is that the course of the functions is irregular near the beginning and the end of the tables, the psychometric functions of the greater judgments not increasing, and those of the smaller judgments not decreasing throughout the whole interval.

In spite of these irregularities, however, there is in the data of all the subjects an interval, which lies in the central parts of the tables, inside of which the psychometric functions behave regularly and are not interrupted in their course by any irregularities. The intensities for which the psychometric functions of the extreme judgments assume the value  $1/2$  are found inside of these intervals. Between these two stimuli lie all the intensities which do not give a probability equal to or exceeding the value one half to either one of the extreme judgments. For this reason it is called the interval of uncertainty. This interval is determined by the method of just perceptible differences and the comparison of the accuracy of sense perception of different subjects, or of the same subject at different times or under different conditions is based on it. It is easy to determine this quantity by interpolation. In determining the lower limit of the interval of uncertainty one picks out the two stimuli which gave to the judgment smaller probabilities just above and just below one half and interpolates new values in this interval by Lagrange's formula, until the quantity to be determined is included in an interval small enough to admit of an interpolation on a straight line. Owing to the regularity of the course of the psychometric functions in the middle of the table only few interpolations by Lagrange's formula are needed for this calculation. We give here a table of the results of the determination

Subject.	Lower Limit of Interval of Uncertainty.	Upper Limit of Interval of Uncertainty.	Length of Interval of Uncertainty.
I.	93.26	100.95	7.69
II.	95.20	99.55	4.33
III.	98.65	100.32	1.57
IV.	95.24	98.26	3.02
V.	93.75	95.83	2.08
VI.	95.82	101.04	5.22
VII.	95.33	100.74	5.44

of the interval of uncertainty by Lagrange's formula for all the persons who served as subjects in our experiments on lifted weights.

The interval of uncertainty is different for different subjects. If we order our subjects by the length of this interval every subject will have a definite place in the series, except when two or more individuals happen to have intervals of uncertainty of the same length. In this latter case the subjects are equally sensitive.

We now turn to the study of the psychometric functions of the equality cases. It is interesting to determine the intensity of the comparison stimulus for which the probability of an equality judgment is greatest. This value can be found easily by means of Table II. We pick out the three greatest values of the table; these values, which we call  $A$ ,  $B$ ,  $C$ , may correspond to the intensities  $x_{R-1}$ ,  $x_R$ ,  $x_{R+1}$ , of the comparison stimulus. The maximum of the probability of the equality judgments is found at the point  $x_R + \xi$  where the quantity  $\xi$  is given by the expression

$$\xi = \frac{A + C}{2(A + C - 2B)}.$$

The maximum probabilities of the equality judgments are found for our seven subjects at the intensities 98.61, 97.57, 100.51, 97.34, 95.89, 99.23 and 96.80. The maximum probability is attained for intensities which are greater than the upper limit of the interval of uncertainty in two cases (subjects III. and V.), from which we conclude that the situation of this maximum has not a definite relation to the interval of uncertainty. It may be remarked that a similar conclusion may be drawn in respect to the arithmetic mean of the equality cases, which represents the abscissa of the center of gravity of the area included between the curve representing the psychometric function of the equality cases and the abscissa.

It is of greater consequence to consider the maximum values of the psychometric functions of the equality judgments. These quantities are found by introducing into Lagrange's formula the values for which the maximum is attained. The



results of this calculation for our seven subjects are 0.4860, 0.2667, 0.1508, 0.2111, 0.1969, 0.3771, 0.3797. These numbers show that the probabilities of the equality judgments are not great; in no case do they exceed one-half and only in the case of subject I does this probability come anywhere near this value, while it never exceeds 0.38 for anyone of the other subjects. This shows that we cannot speak of a point of equality in any absolute sense of the word, because we have to require that such a point must give to the equality judgments probabilities greater than one-half.

Let us compare the orders which we obtain when the subjects are ordered by the length of their intervals of uncertainty and by the maximum probabilities which they give to equality judgments. We obtain the following arrangements of our seven subjects.

Subjects Ordered by  
Maximum Probability of  
Equality Judgments.

I.  
VII.  
VI.  
II.  
IV.  
V.  
III.

Subjects Ordered by  
Length of  
Interval of Uncertainty.

I.  
VII.  
VI.  
II.  
IV.  
V.  
III.

The order of all the subjects is the same in both cases, and we conclude from this fact that the maximum probability of the equality judgments can be used just as well as a measure of the accuracy of sense perception as the interval of uncertainty. It is, therefore, possible to base a comparison of the accuracy of sense perception on the equality cases alone, a fact which may be surprising if one remembers that these judgments are so much of a difficulty in the customary treatment of psychophysical measurement methods that some investigators advised the absolute suppression of these judgments.

The fact that there exist two quantities each one of which may be used for the purpose of comparing the accuracy of sense perception is interesting, because these quantities are independent of one another and are derived from different data. The limits of the interval of uncertainty depend directly only

on the probabilities of one of the extreme judgments, on those of the equality judgments and of the other extreme judgment only indirectly through the sum of the probabilities which must be equal to one. The probabilities of the equality judgments have, therefore, no direct influence at all on the length of the interval of uncertainty. The maximum probability of the equality cases, on the other hand, is entirely independent from the probabilities of the extreme judgments, and the methods of calculation, which lead to the determination of the maximum probability of the equality judgments and of the interval of uncertainty, have nothing in common. We will conclude from this fact that there exist certain relations between the different psychometric functions, which must be investigated.

It is not possible to base the definition of the point of subjective equality on the equality judgments. There remains the possibility of basing this definition on the extreme judgments by defining the point of subjective equality as that intensity of the comparison stimulus for which the probabilities of the extreme judgments are equal. If  $r$ ,  $s$ ,  $t$  are the probabilities of the smaller, equal and greater judgments, this definition of the point of subjective equality implies that  $r = t$ ; no specification is made in regard to the values of either one of these quantities. The approximate determination of this point from a table of the psychometric functions offers no difficulty. We find in the table the stimulus  $\alpha_1$  which gives a higher probability to the smaller than to the greater judgments and which is immediately followed by the stimulus  $\alpha_2$  which gives a higher probability to the greater than to the smaller judgments. Let the corresponding probabilities of the smaller judgments be  $y_1, y_2$  and those of the greater judgments  $z_1, z_2$ . If the interval between the points  $\alpha_1$  and  $\alpha_2$  is small enough to admit of an interpolation on a straight line the abscissa of the point of intersection of the curves representing the psychometric functions of the extreme judgments is given by the formula

$$\alpha - \alpha_1 = \frac{(\alpha_1 - y_1)(\alpha_2 - \alpha_1)}{y_2 - y_1 + z_1 - z_2}.$$

We give here the results of this calculation for the seven sub-

jects of our experiments on lifted weights with reference to the limits of the interval of uncertainty.

Subject.	Lower Limit of Interval of Uncertainty.	Point of Subjec- tive Equality.	Upper Limit of Interval of Uncertainty.
I.	93.26	97.20	100.95
II.	95.20	97.36	99.55
III.	98.65	99.46	100.32
IV.	95.24	96.62	98.26
V.	93.75	94.66	95.83
VI.	95.82	98.42	101.04
VII.	95.33	98.59	100.74

These numbers show that the point of subjective equality defined in terms of the probabilities of the extreme judgments lies always inside of the interval of uncertainty and a closer inspection reveals the fact that it coincides very closely with the centre of this interval. Let us divide the distance of the point of subjective equality from the lower limit of the interval of uncertainty by the length of this interval. We obtain the following values for seven subjects: 0.49, 0.50, 0.51, 0.54, 0.56, 0.50 and 0.40, the average of which is 0.50. We conclude from this, that the point of subjective equality coincides with the center of the interval of uncertainty.<sup>1</sup> The difference between the point of subjective equality and the standard gives the constant error which in the case of our experiments is due to the order in which the weights were presented to the subject, *i. e.*, to the so-called time error. We find the following values

<sup>1</sup>This proposition is the justification of all the investigations in which equal appearing stimuli are found by the method of just perceptible differences, as, *e. g.*, in Miss Cook's investigation on the illusion of filled and unfilled tactual spaces. Only the just imperceptible positive and negative differences were determined in her experiments (the just perceptible differences were used only in some series) and the averages of these values were taken. This average determines the center of the interval of uncertainty, *i. e.*, the point of subjective equality which of course has to be determined in a study of an illusion. Miss Cook, however, did not use the method of just perceptible differences in its traditional form but in Sanford's variation, so that one can not make the positive statement that she really determined the stimulus which gives equal probabilities to the extreme judgments. It is not likely that the influence of her variation on the final result is great, but it is inconvenient to have to deal with results which escape an exact interpretation. Cf. Helen Dodd Cook, 'Die taktile Schätzung von ausgefüllten und leeren Strecken,' *Archiv f. d. ges. Psychologie*, 1910, Vol. 16, pp. 451-456.

for this quantity 2.80, 2.65, 0.54, 3.38, 5.34, 1.58 and 1.41. All these differences being negative we clearly have to deal with an over-estimation of the second weight.

An extremely important conclusion may be drawn from this proposition. The similarity of the method of just perceptible differences with the procedure by which we determine unknown empirical quantities by measurement was noticed by several investigators, although one could not agree as to the exact point of similarity. An accurate understanding of the method of just perceptible differences is of the greatest importance for the general theory of empirical measurements. In determining the unknown weight of a body, for example, we start after a preliminary rough determination of the approximate value by comparing the body with known weights which are too great and gradually reduce the difference until no difference is noticed between the two weights. We keep on diminishing the comparison weight until a point is found where the comparison weight is found to be too small. The average of these results is taken as a determination of the unknown weight of the body. The whole process is repeated in the ascending direction, which gives another determination. Several such determinations have to be made for the purpose of obtaining an exact determination and the final result is found by the arithmetic mean of all the individual results.

The fact that we pick out the arithmetic mean as the final determination of the quantity to be measured implies that we regard it as the best value obtainable. This is the meaning of the proposition that the arithmetic mean has to be regarded as the most probable value of a set of measurements of an empirical quantity. Taking this proposition as a principle one may deduce from it the method of least squares, which is a set of rules for finding the most probable values of empirical quantities and the limits of the exactitude of their determination, if the principle of the arithmetic mean is granted. This is the way of reasoning followed by Gauss in his first deduction of the method of least squares. The theory of errors of observation based on this principle has stood the test of practice for more than a hundred years, and it may be granted that the principle of the

arithmetic mean seems to be a very obvious one, but it can not be denied that it makes the impression of being artificial. A great number of attempts were made to demonstrate it, but no proof could be given without introducing some other proposition which is equivalent to the principle of the arithmetic mean. As it was known that other definitions of the most probable value of a set of measurements of an empirical quantity lead to entirely different rules of calculation, one began to suspect that the theory of errors of observation required the introduction of a principle of empirical origin, just as much as the use of ordinary geometry in geodesy and astronomy requires the proposition that empirical space is a three-dimensional Euclidean manifoldness.

It is only a step in the same direction of development to prefer an empirical verification of the law of distribution, which follows from the principle of the arithmetic mean, to any *a priori* deduction, because such a demonstration must necessarily start from some other hypothesis. This is the view expressed by H. E. Faye and H. Laurent. For those who maintained this view it became necessary to show that the distribution of the individual results in a set of measurements is such as to warrant the application of the principle of the arithmetic mean. This required observations on actual distributions of errors, which were given in the works of Bessel, C. S. Peirce, Guarducci, Laurent, Helmert, F. Y. Edgeworth and others. In the investigations referred to a satisfactory agreement between theory and practice was observed, but this position became seriously endangered, not to say untenable, by the discovery that most empirical distributions show an essential asymmetry, symmetry being found as an exception only in very rare cases. One might suspect that the symmetry observed in sets of measurements is merely a chance result or the effect of some peculiar conditions, as it very likely is the case of Peirce's observations.<sup>1</sup>

<sup>1</sup>C. S. Peirce, 'On the Theory of Errors of Observation,' Report of the U. S. Coast and Geodetic Survey, 1870, made a very extended series of observations on reaction time using the Hipp chronoscope. His results, curiously enough, show a symmetrical distribution, although it is one of the best established facts that the distribution of reaction times is asymmetrical. It is of course out of question that Peirce did not observe or report correctly and it is all the more

But granting even that there is no flaw in any of these observations, one must ask how it comes that the errors of observations show a symmetrical distribution while all the other empirical distributions are essentially and normally asymmetrical. Neither an empirical nor a mathematical justification of the principle of the arithmetic mean is at hand and the method of least squares seems to hang in the air. We are confronted with the shocking situation that a proposition is triumphantly borne out by an immense indirect experience and that it can be proved neither by mathematical deduction nor by direct experience.

The only case where there does not exist a doubt as to the justification of the principle of the arithmetic mean is that of the empirical determination of unknown probabilities, because the arithmetic mean is the most probable value of the quantity to be determined according to the theorems of the calculus of probabilities. This fact has to be utilized for the theory of errors of observation. A single determination of an empirical quantity is obtained by following a strict rule which determines which intensity has to be put down as the result of an individual measurement. On the basis of any such definition one can set up an expression which has the character of a mathematical expectation and it follows that the most probable value of a set of individual determinations is given by their arithmetic mean. The arithmetic mean, therefore, is the most probable value in all those measurements in which a systematic procedure is strictly followed.

In those cases where the individual determinations were obtained by following the procedure described above we can go a step further. The first result of such a determination is what we call the just imperceptible positive difference, and the point where the balance indicates a difference between the two bodies is the just perceptible negative difference. In the ascending series we determine first the just imperceptible negative, and then the just perceptible positive difference. Since

necessary to explain his result, because Pizzetti, '*Ifondamenti matematici per la critica dei risultati sperimentali*,' 1892, attributes great weight to Peirce's observations. The symmetry of the distribution in Peirce's results is due to the mixture of different distributions, as will be shown in a paper to be published in the near future.

the final average of the set is not influenced by grouping them and taking the arithmetic mean of the averages of the groups we may combine first the just perceptible and the just imperceptible positive, and then the just perceptible and the just imperceptible negative differences, obtaining in the first case the threshold in the direction of increase and in the second the threshold in the direction of decrease. The arithmetic mean of these two quantities is identical with the average of all the individual results, from which it follows that the final determination of the value of an empirical quantity coincides with the center of the interval of uncertainty. We conclude that we determine by our empirical measurements those intensities for which the probability of the judgment greater is equal to that of the judgment smaller.

We thus obtain the remarkable result that the foundations of the theory of errors of observation are found in the theory of psychophysical measurement. The principle of the arithmetical mean as the most probable value of a set of empirical measurements since more than a hundred years proved refractory to all attempts at a purely mathematical demonstration and empirical demonstrations lack finality because they do not show the cause of the symmetry of this distribution in the face of an indefinite number of asymmetrical distributions. The cause of this failure is to be sought in the notion of the probability of an error of certain size, which is the basis of the theory of errors of observations. The process by which we arrive at assigning a definite value to an empirical quantity is very complicated and requires further analysis. This analysis, however, can be given only by means of the notion of the probability of a judgment, which is entirely foreign to the theory of observations, because this science considers only the result of the process of measuring. The notion of the probability of a judgment belongs to psychology as well as the analysis of the conditions which influence it. It was assumed in most, not to say in all the treatises on the theory of psychophysical measurement that this science has to depend on the theory of errors of observations, which furnishes the data on which we have to build. This is not the case. The relation of these two sciences is just the opposite. Psychology furnishes the notion of the probability of a judg-

ment and thus opens the way to an understanding of the principle of the arithmetical mean, and it also furnishes the notion of the accuracy of sense perception. The theory of errors of observation reciprocates by offering problems and refined observations, which properly belong to the psychology of sense perception.

We now turn to the study of the treatment of the psychometric functions by means of definitive hypotheses. Let  $f(x)$ ,  $g(x)$ ,  $h(x)$  be the functions which represent the smaller, equal and greater judgments. Each one of these functions also depends on a number of parameters which must be determined from the data of observation, so that we may write explicitly  $f(x; a_1, b_1, c_1, \dots)$ ,  $g(x; a_2, b_2, c_2, \dots)$ ,  $h(x; a_3, b_3, c_3, \dots)$ . Generally the number of observations is greater than the number of constants to be determined, so that they must be determined from an overdetermined set of equations. Owing to the fact that empirical determinations of unknown probabilities are not exact, certain discrepancies between the different results will arise which must be eliminated by an adjustment. The procedure to be used for this purpose will become clear by the following considerations. The individual observations consist of empirical determinations of certain unknown probabilities, the probable errors of which are given by the theorem of Bernoulli. Let  $s_R$  experiments be made with the comparison stimulus  $\alpha_R$ ,  $m_R$  of which may have resulted in the judgment smaller,  $o_R$  in the judgment equal and the rest  $n_R = s_R - (m_R + o_R)$  in the judgment greater. The fractions  $m_R/s_R$ ,  $o_R/s_R$ ,  $n_R/s_R$  are the most probable determinations of the underlying probabilities and the probability that these results are affected by an error of the size  $\gamma$  is given by the expression

$$\frac{h}{\sqrt{\pi}} e^{-h^2 \gamma^2},$$

where  $h$ , the coefficient of precision, is given for the three probabilities by

$$\sqrt{\frac{s_R^3}{2m_R(s_R - m_R)}}, \quad \sqrt{\frac{s_R^3}{2o_R(s_R - o_R)}}, \quad \sqrt{\frac{s_R^3}{2n_R(s_R - n_R)}}$$



respectively. The results of the experiments with every comparison stimulus, therefore, give three equations of the form

$$f(x_R; a_1, b_1, c_1, \dots) = \frac{m_R}{s_R},$$

$$g(x_R; a_2, b_2, c_2, \dots) = \frac{o_R}{s_R},$$

$$h(x_R; a_3, b_3, c_3, \dots) = \frac{n_R}{s_R},$$

which must satisfy the condition equation

$$f(x) + g(x) + h(x) = 1.$$

One might believe that a condition equation must be entered for every observation, but this is not the case, because the sum of all the psychometric functions must be equal to one for any value of the comparison stimulus, from which it follows that this sum must be identically one. This simplifies the calculation considerably, because one of the psychometric functions may be determined as the difference of one minus the sum of the two other functions, so that the adjustment of the observations need be carried through only for two of them.

Let us suppose that  $g(x)$  is determined by  $f(x)$  and  $h(x)$ . We then have only two observations for every stimulus, for which the probabilities of the different judgments were observed. This gives a system of equations for the determination of the quantities  $a_1, b_1, c_1, \dots$  which occur only in the equations originating from  $f(x)$ , and  $a_3, b_3, c_3, \dots$  occurring only in the equation originating from  $h(x)$ . The whole system may be solved by treating these two groups separately, and since the constitution of both groups is the same, we may confine the theory to showing how one of them may be solved. Let us take the group which contains the constant of  $f(x)$  and refer to them by the letters  $a, b, c, \dots$ , omitting the indices.

We introduce here for the sake of simplifying the calculation the assumption that the constants  $a, b, c, \dots$  occur in  $f(x)$  in such a way as to form a linear complex

$$\alpha_R a + \beta_R b + \dots$$

where the quantities  $\alpha_R, \beta_R, \dots$  depend on the intensities of the comparison stimulus and are, therefore, different for different intensities. We also assume that there exists an inverse function  $F(x)$  so that

$$\alpha_R a + \beta_R b + \dots = F\left(\frac{m_R}{s_R}\right).$$

We obtain under these conditions an overdetermined system of linear equations the errors in the determinations of the constants of which follow the exponential law. From this it follows that the most probable values of the unknown quantities must be found by the method of least squares, each equation being put down with the proper weight. Only the fractions  $p_R = m_R/s_R$  are directly determined, their coefficient of precision  $h_R$  being given by the theorem of Bernoulli. The coefficient of precision in the determination of  $F(p_R)$  may be called  $H_R$  and is found by the formula

$$\frac{1}{H_R^2} = \frac{\left(\frac{dF}{dp}\right)_{p=p_R}}{h_R^2}.$$

Since the weight of every observation equation is directly proportional to the square of the coefficient of precision in the determinations of  $F(p)$ , we have all the quantities which we need for the solution of our system which has this form

$$\alpha_1 a + \beta_1 b + \dots = F_{(p_1)} \text{ with the weight } P_1,$$

$$\alpha_2 a + \beta_2 b + \dots = F_{(p_2)} \quad \text{“} \quad \text{“} \quad \text{“} \quad P_2,$$

$$\dots \dots \dots$$

$$\alpha_i a + \beta_i b + \dots = F_{(p_i)} \quad \text{“} \quad \text{“} \quad \text{“} \quad P_i.$$

From these equations the normal equations are derived in the usual way and their solution gives the most probable values of the constants of the function  $f(x)$ . Introducing these values in the observation equations one obtains the deviations of the calculated from the observed results and the sum of the squares of these deviations permits to state the accuracy obtained in the determination of the constants of the psychometric functions.

The method of calculation explained here is based on two

suppositions which must not be overlooked. The first supposition says that the nature of the dependence between the intensity of the comparison stimulus and the probabilities of the different judgments is known, whereas the second supposition specifies this assumption still further by admitting the existence of the function  $F(p)$  with all its qualities. This assumption is not necessary, because one can solve the problem without it, but it is extremely convenient and facilitates the calculation considerably. A further argument in favor of this restriction of the problem is the fact that the existence of the function  $F(p)$  was tacitly assumed in all previous investigations. No objection can be raised against this supposition, unless one proposes to solve the general problem, an undertaking which is more laborious than difficult.

The question as to the justification of the first hypothesis offers an entirely different aspect. A hypothesis on the psychometric functions is valid only when it expresses the actual dependence of the probabilities of the different judgments on the intensity of the comparison stimulus. The nature of this dependence is not known and cannot possibly be deduced by any considerations *a priori*. It is quite obvious that a function cannot be the object of any immediate experience, but that it must be found by regularities in the results of observation. No such knowledge is at hand at present, so that one hypothesis about the psychometric functions is just as arbitrary as any other. This is the peculiar difficulty of this problem, that one is forced either to make a hypothesis which is not more justified than any other or that one must treat the results by Lagrange's (or some analogous) formula of which one knows positively that it cannot be correct and which takes in errors which are absolutely unknown as to their size and sign.

It seems to be best to test the experimental material at hand by different hypotheses and not to regard any one of them as final, no matter what its success may have been. What we really need is a standard by which we can judge the different hypotheses, because if we have one, we can discard certain mathematical expressions as unsuitable to represent the psychometric functions. The treatment of the same data by different hypotheses furnishes

valuable indications. If the constants of the psychometric functions are known we can calculate the probabilities of the judgments according to the different hypotheses and compare them with the results of the observations. The sum of the squares of these deviations is the criterion of the value of a hypothesis. It is not possible to arrive at a final conclusion in this way, because an infinity of hypotheses would have to be gone through, but it may be decided which one of a certain set of mathematical expressions is best suited to represent the psychometric functions. The number of functions which for practical purposes come under consideration as hypotheses on the dependence of the probabilities of the judgments on the intensities of the comparison stimulus is naturally rather limited. The process of calculating the constants of the psychometric functions is rather laborious and it becomes the more so the more complicated the hypotheses are. For this reason one will not be inclined to take up the study of very complicated functions, unless there are strong arguments in favor of them. It may be expected that among these functions there is one which is more suitable as a hypothesis about the psychometric functions than all the others. This hope is as well compatible with the view that the form of the psychometric functions is the same for all subjects, as with the more conservative view that these functions differ for different individuals and perhaps even for the same individual at different times.

We may regard a hypothesis on the psychometric functions merely as a mathematical expression which fits one set of experimental data well, and another, perhaps, less satisfactorily, but we consider it from the start as extremely unlikely that an expression can be found which fits all data equally well. One may support this view by referring to the individual differences between the different subjects, which seem to exclude any such regularity. If, contrary to expectation, such an expression is found we will not have to change our views materially and benefit by this discovery for the facilitation of future work.

The problem which confronts us in the study of the psychometric functions is similar to the problem of determining the probability of dying as a function of age. A mathematical ex-

pression which gives this dependence is called the biometric function. These functions are *a priori* just as unknown as the psychometric functions are and the same difficulties are encountered in their *a posteriori* determination, but experience shows that there exists a formula, the so-called formula of Gompertz-Makeham, which has done better service than any other formula tried for this purpose. One therefore expects a similarly satisfactory result for the future and one naturally turns to this formula if new material is to be treated. The modern view about the biometric functions is similar to the one which we gave for the psychometric functions, namely that it is impossible to find an expression which fits all data equally well, a view which is not only supported by past experience, but which also may be backed up by the argument that the conditions under which men live are so different that the existence of any such regularity seems very unlikely. Experience must show whether there is less difference in the psychological make-up of people, but meanwhile we may undertake to find out how different hypotheses on the psychometric functions work out in their application to the results of observation.

We will consider here two hypotheses on the psychometric functions, in which the probabilities of the equality cases are expressed in terms of those of the extreme judgments. The psychometric function of the smaller judgments may be represented by the expression

$$f(x) = \frac{1}{\sqrt{\pi}} \int_{h_1(x-a_1)}^{\infty} e^{-t^2} dt$$

This expression is admissible as a hypothesis on the psychometric function of the smaller judgments, because it decreases with increasing intensity of the comparison stimulus and approaches the limit 1 for  $x = -\infty$  and the limit 0 for  $x = \infty$ . One easily sees that the expression

$$h(x) = \frac{1}{\sqrt{\pi}} \int_{-\infty}^{h_2(x-a_2)} e^{-t^2} dt$$

is admissible as a hypothesis on the psychometric function of the greater judgments. The probabilities of the equality

judgments are given by

$$g'(x) = \frac{1}{\sqrt{\pi}} \int_{h_1(x-a_1)}^{h_2(x-a_2)} e^{-t^2} dt.$$

The three functions  $f(x)$ ,  $g'(x)$ ,  $h(x)$  contain only the constants  $a_1$ ,  $a_2$ ,  $h_1$  and  $h_2$  and they are fully determined by them. These constants must be determined in such a way as to fit the data from which they are deduced as well as possible.

This hypothesis may be called the  $\Phi(\gamma)$ -hypothesis. It is remarkable and well known for the fact that G. E. Mueller uses it in his method of constant stimuli. Mueller starts from the notion of a threshold which is subjected to chance variations, the frequency of which is a function of their size. The mathematical expression for the probabilities of the variations as depending on their size is called their law of distribution. Mueller and his followers assume the exponential law, which frequently but not very appropriately is called the Gaussian law, to hold good for the distribution of the threshold. The ordinate of the maximum of this function is an axis of symmetry, which gave rise to the well-known discussion whether it was admissible to make the assumption that the variations of the threshold follow a symmetrical law of distribution. This objection was strengthened by the fact that all empirical distributions studied until now show an essential asymmetry which is sometimes small but sometimes very considerable indeed. It is not possible to say that the discussion of this problem was very fruitful of important results.

The question as to the symmetry of the law of distribution has the following meaning for the psychometric functions.  $f(x)$  assumes the value  $1/2$  for  $x = a$ . Keeping in mind that the probability integral from zero to any positive limit is equal to that from zero to the same negative limit, we see that  $f(a - x)$  and  $f(a + x)$  are symmetric to the value  $1/2$ . The curve representing  $f(x)$  may be divided into two parts each one of which goes over into the other by being mirrored at the lines  $y = 1/2$  and  $x = a$ , the order of this process being indifferent. It is quite obvious that this implies a very special hypothesis on the psychometric functions, but any other hypothesis has to be equally specific, and if one is thoroughly imbued with the con-

viction that nothing is definitely decided by a provisory acceptance of a hypothesis, one will not attribute too much importance to this question.

It is necessary for the practical application of this method to have a table of the values of this function. One either may construct a table similar to the well-known fundamental table for the method of right and wrong cases or one may use a table of the probability integral. Fechner's table is very convenient for working out sets of 25, 50 or 100 experiments, but in all the other cases it is more convenient to use a table of the proba-

TABLE III.

$p$	$\gamma$	Difference.	$p$	$\gamma$	Difference.
0.50	0.0000	177	0.76	0.4994	230
0.51	0.0177	178	0.77	0.5224	236
0.52	0.0355	177	0.78	0.5460	242
0.53	0.0532	178	0.79	0.5702	249
0.54	0.0710	178	0.80	0.5951	257
0.55	0.0888*	179	0.81	0.6208	265
0.56	0.1067*	180	0.82	0.6473	274
0.57	0.1247	180	0.83	0.6747	284
0.58	0.1427*	182	0.84	0.7031*	298
0.59	0.1609	183	0.85	0.7329	310
0.60	0.1792*	183	0.86	0.7639	326
0.61	0.1975	185	0.87	0.7965	347
0.62	0.2160	186	0.88	0.8308	365
0.63	0.2346*	189	0.89	0.8673	389
0.64	0.2535	190	0.90	0.9062	418
0.65	0.2725	191	0.91	0.9480*	455
0.66	0.2916*	195	0.92	0.9935*	500
0.67	0.3111	196	0.93	1.0435*	558
0.68	0.3307	199	0.94	1.0993*	637
0.69	0.3506	202	0.95	1.1630*	750
0.70	0.3708	205	0.96	1.2380*	920
0.71	0.3913	208	0.97	1.3300*	1220
0.72	0.4121	212	0.98	1.4520*	1930
0.73	0.4333	216	0.99	1.6450	
0.74	0.4549	220	1.00	$\infty$	
0.75	0.4769	225			

bility integral. The tables of Kaempfe<sup>1</sup> and of Bruns<sup>2</sup> are easily accessible to psychologists and they have the great advantage that the interval of the table is very small. The values of the psychometric function calculated from the table of Bruns do not always coincide with the data of the table of Fechner, which is reprinted in most of the treatises on psychophysical measure-

<sup>1</sup> B. Kaempfe, *Psychologische Studien*, 1893, Vol. 9.

<sup>2</sup> H. Bruns, *Wahrscheinlichkeitsrechnung und Kollektivmasslehre*, 1906.

ment methods, and it seems advisable to give here a table of the values calculated from the table of Bruns. The arrangement of this table is identical with that of the fundamental table for the method of right and wrong cases; values marked by an asterisk (\*) differ from the corresponding values in Fechner's table. Owing to the symmetry of the function it is sufficient to give the values between 0.50 and 1.00.

The next step consists in finding the formula for the weights of the observation equations. Following the line of argumentation given above we find for the weight of the observation with a comparison stimulus

$$P = \frac{1}{2\pi} \cdot \frac{se^{-2\gamma^2}}{p(1-p)}$$

where  $s$  is the number of experiments made. This formula corresponds to the expression which G. E. Mueller gave for the weight of an observation equation, but it differs from it in such a way as to give results which are greater than the corresponding values of Mueller. An analysis of this formula shows (1) that the function representing the weight of the observation equations has a maximum at  $p = \frac{1}{2}$  and that the ordinate of this maximum is an axis of symmetry of the function, and (2) that the function assumes the value zero for  $p = 1$  and for  $p = 0$ , showing that observations which gave the frequency 0 or 1 for one of the extreme judgments are without influence on the determination of the constants of the psychometric function of this judgment, from which it follows that these results simply may be omitted. It is best to use comparison stimuli which give probabilities not differing much from  $\frac{1}{2}$ , because these observations come down with the greatest weight. It is convenient to have a table of these weights, which owing to the symmetry of the function representing them need cover only one of the intervals from 0 to 0.5 or from 0.5 to 1. Table IV. contains these values for the interval from 0.5 to 1; the arrangement and the use of this table is identical with that of Mueller's table.

The process of setting up and solving the normal equations derived from the observation equations is well known and need



TABLE IV.

WEIGHTS OF THE OBSERVATION EQUATIONS ACCORDING TO THE  
 $\Phi(\gamma)$ -HYPOTHESIS.

$\phi$	$P$	Difference.	$\phi$	$P$	Difference.
0.50	1.000	0	0.76	0.832	14
0.51	1.000	1	0.77	0.818	15
0.52	0.999	1	0.78	0.803	16
0.53	0.998	2	0.79	0.787	17
0.54	0.996	1	0.80	0.770	18
0.55	0.995	3	0.81	0.752	19
0.56	0.992	3	0.82	0.733	20
0.57	0.989	4	0.83	0.713	19
0.58	0.985	4	0.84	0.694	24
0.59	0.981	4	0.85	0.670	24
0.60	0.977	5	0.86	0.646	25
0.61	0.972	5	0.87	0.621	26
0.62	0.967	7	0.88	0.595	28
0.63	0.960	6	0.89	0.567	29
0.64	0.954	7	0.90	0.538	32
0.65	0.947	7	0.91	0.506	34
0.66	0.940	8	0.92	0.472	37
0.67	0.932	9	0.93	0.435	39
0.68	0.923	9	0.94	0.396	44
0.69	0.914	10	0.95	0.352	48
0.70	0.904	10	0.96	0.304	55
0.71	0.894	11	0.97	0.249	62
0.72	0.883	12	0.98	0.187	78
0.73	0.871	12	0.99	0.112	112
0.74	0.859	13	1.00	0.000	
0.75	0.846	14			

not be described here, but a few remarks may prove useful. The opinion is very widespread that this process is difficult or at least very laborious. This is not the case if the computations are arranged properly. With all the necessary checks the calculations ought not to take more than two hours even if one has only little practice in this work. A scheme of calculation which has stood the test of practical application is given in the *Archiv f. d. ges. Psychologie*, Vol. 16.

We give here the results of the computation for the seven subjects in our experiments on lifted weights. The first two columns of Tables V. and VI. give the constants of the psychometric functions and the columns under the headings  $S_1$  and  $S_2$  give the lower and upper limit of the interval of uncertainty. These values may be compared with the results of calculation and observation by the method of just perceptible differences and by Lagrange's formula of interpolation. The results of

TABLE V.

CONSTANTS OF THE PSYCHOMETRIC FUNCTION OF THE SMALLER JUDGMENTS  
BY THE  $\Phi(\gamma)$ -HYPOTHESIS. LOWER LIMIT OF THE INTERVAL  
OF UNCERTAINTY.

Subject.	$h_1$	$c_1$	$S_1$	Method of Just Perceptible Differences.		Interpolated by <i>Lagrange's</i> Formula.
				Calculated.	Observed.	
I.	0.125777	11.7398	93.34	93.49	93.30	93.26
II.	0.105252	10.0074	95.08	94.98	94.87	95.20
III.	0.138920	13.6010	97.91	97.88	97.85	98.65
IV.	0.123357	11.7730	95.44	95.56	95.39	95.24
V.	0.127380	12.0011	94.22	94.57	94.47	93.75
VI.	0.110215	10.5023	95.29	95.20	95.31	95.82
VII.	0.114453	10.9650	95.80	96.74	95.79	95.33

these different methods agree so well that no further discussion is needed.

It is a notorious fact that in former investigations the method of constant stimuli did not give the same results as the method of just perceptible differences. This was due to an imperfect understanding of the method of just perceptible differences and one may safely say that the difficulties of the method of constant stimuli were largely due to those of the method of just

TABLE VI.

CONSTANTS OF THE PSYCHOMETRIC FUNCTION OF THE GREATER JUDGMENTS  
BY THE  $\Phi(\gamma)$ -HYPOTHESIS. UPPER LIMIT OF THE INTERVAL  
OF UNCERTAINTY.

Subject.	$h_2$	$c_2$	$S_2$	Method of Just Perceptible Differences.		Interpolated by <i>Lagrange's</i> Formula.
				Calculated.	Observed.	
I.	0.136113	13.5676	99.68	99.60	99.45	100.95
II.	0.110945	11.0138	99.27	98.71	98.83	99.55
III.	0.145240	14.4346	99.39	99.58	99.28	100.32
IV.	0.117995	11.5917	98.24	98.24	98.08	98.26
V.	0.115708	11.2424	97.16	97.35	97.14	95.83
VI.	0.114995	11.5862	100.75	100.33	99.86	101.04
VII.	0.115465	11.6816	101.17	99.63	99.86	100.74

perceptible differences. As long as one did not see that the result of this latter method could be defined in terms of the probabilities of the different comparison stimuli, one could not possibly use the same material for a test of both methods, and

it is very difficult to obtain different sets of results under exactly the same conditions. This is particularly true with reference to experiments made by the method of just perceptible differences in its traditional form and those of the method of constant stimuli, because in the first case one has to present the stimuli in a certain order (ascending or descending), whereas they may be given in random order in the method of constant stimuli. For this reason it is not possible in the method of just perceptible differences to keep the subject in ignorance as to the direction in which the stimuli are varied. This circumstance causes a difference in the attitude of the subject which must influence the judgment, and for this reason it is not very likely that the experimental data obtained by the two methods are strictly comparable.

Differences in the conditions of the experiments may be detected in two ways, by introspection or by a difference in the objective results. We take the view that introspective evidence against a set of experiments makes it suspect, but the absence of any such objection does not put the value of the material beyond doubt. This is the view which is taken in the theory of observations where a set of observations must not be judged off-hand but only on the basis of a minute examination. Differences in the objective results of psychophysical experiments are differences in the values of the observed probabilities and we may call the conditions of two groups of experiments materially different if the judgments have not the same probabilities for the comparison of the same stimuli. Arrangements which are not identical, but which do not interfere with the values of the probabilities of the different judgments, are said to be only formally different. The method of just perceptible difference when looked at as a method of calculation is only formally different from the method of constant stimuli, because, as Tables V. and VI. show, both methods give the same results. The method of just perceptible differences in its traditional form, however, requires results of special kind which are materially different from those obtained and used in the method of constant stimuli. This accounts for the differences thought to exist between these two methods.

The problem of finding the relation between the different psychophysical methods has a well-defined meaning only if one refers to results obtained under conditions which are not materially different. The problem of psychophysics in general is to determine the influence of the conditions of the experiments on our judgment, those conditions being of primary importance which depend on the state of the subject. Various methods have been devised for the purpose of this analysis, the formal character of which has to be perfectly understood before a conclusion can be drawn as to the material difference or identity of the conditions in the different sets of observations. We may express this idea by saying that the purpose of the psychophysical measurement methods is an analysis of the material conditions which determine our judgment on the comparison of stimuli, but for this purpose an understanding of the formal character of these methods is needed.<sup>1</sup>

When the constants of the psychometric functions are known one may calculate the probabilities of the different judgments for all intensities of the comparison stimulus. The results of this calculation are given in Table VII. The numbers under the heading 'observed values' give the difference between the

<sup>1</sup> The distinction between formal and material conditions of an experiment was not favorably criticized in the discussion following the reading of the paper, perhaps because it was not as clearly presented as it might have been. This distinction, however, is absolutely indispensable for a proper understanding of the psychophysical methods. Experience shows that the results of the method of just perceptible differences coincide with those of the method of constant stimuli if the same material is used for this test, and that they are different if different materials are used. There is obviously no reason why the results should agree in one case and not agree in the other, unless there are some similarities between the two methods, which are counteracted by the differences of the conditions under which the material was obtained. In the monograph on 'The Application of Statistical Methods to the Problems of Psychophysics' the emphasis was laid on the formal identity of the method of just perceptible differences with the error methods, because it was a new observation that both methods give the same result if they are tested on the same material. Mr. G. Geiger (*Zeitschrift f. Psychologie*, 1910, Vol. 54, pp. 540-542) in his review of this book and Miss H. D. Cook in her treatise on 'Die taktile Schätzung von ausgefüllten und leeren Strecken,' *Archiv f. d. ges. Psychologie*, 1910, Vol. 16, p. 455, justly point out that the differences between the experiments made according to these two methods are not sufficiently dwelt upon. This remark is perfectly to the point and shows the importance of making the distinction between formal and material conditions of an experiment.

TABLE VII.

VALUES OF THE PSYCHOMETRIC FUNCTIONS.

COMPARE CHART 2.

Comparison Stimulus.	Smaller.		Equal.		Greater.	
	Calculated.	Observed.	Calculated.	Observed.	Calculated.	Observed.
80	0.9876		0.0011		0.0113	
82	0.9742		0.0030		0.0228	
84	0.9504	—0.0171	0.0075	+0.0147	0.0421	+0.0023
86	0.9117		0.0172		0.0711	
88	0.8540	+0.0082	0.0357	—0.0113	0.1103	+0.0030
90	0.7752		0.0682		0.1566	
92	0.6767	+0.0233	0.1200	—0.0089	0.2033	—0.0144
94	0.5639		0.1946		0.2415	
96	0.4450	+0.0033	0.2920	+0.0013	0.2624	—0.0046
98	0.3319		0.4076		0.2605	
100	0.2320	—0.0009	0.5319	—0.0030	0.2361	+0.0039
102	0.1515		0.6532		0.1953	
104	0.0922	+0.0034	0.7605	+0.0551	0.1473	—0.0584
106	0.0520		0.8465		0.1015	
108	0.0272	—0.0116	0.9091	—0.0047	0.0637	+0.0163
110	0.0132		0.9504		0.0364	
112	0.0059		0.9752		0.0189	

calculated and the observed values of the probabilities, the sign being determined so as to make the arithmetic sum of the terms equal to the observed value. The differences between the calculated and the observed values are very small and some of them are negative and some positive, both signs being distributed quite irregularly throughout the table. The data of this table may be represented graphically, as it is shown in Chart 2. The construction of the curves in this chart is the same as that of Chart 1, so that it need not be explained again. It is seen at a glance that the course of the curves is regular throughout the table and that the ascent and descent of the curves representing the psychometric functions of the extreme judgments is not interrupted by any secondary elevations.

We now turn to the study of another hypothesis on the psychometric functions. One sees immediately that an expression of the form

$$f(x) = \frac{1}{2} - \frac{1}{\pi} \arctan(ax + b)$$

may represent the psychometric function of the smaller judgments, because it approaches the values zero and one asymptotically.

totically and decreases for increasing values of the comparison stimulus. For similar reasons is it admissible to suppose that such a function may represent the psychometric function of the greater judgments, if the sign and the constants of the term  $\arctan$  are determined appropriately. This hypothesis leads to a computation similar to that of the first hypothesis and the necessary formulæ may be easily found by the considerations given above,<sup>1</sup> but for our present purpose only the question is of importance whether this hypothesis agrees better with experience than the  $\Phi(\gamma)$ -hypothesis.

TABLE VIII.

SUMS OF THE SQUARES OF THE DEVIATIONS OF THE CALCULATED  
FROM THE OBSERVED VALUES.

Subject.	Arctan-Hypothesis.			$\Phi(\gamma)$ -Hypothesis.		
	Smaller.	Greater.	Equal.	Smaller.	Greater.	Equal.
I.	0.019879	0.016931	0.024358	0.001738	0.022826	0.019738
II.	0.010243	0.010667	0.011302	0.001060	0.003492	0.003934
III.	0.019196	0.016839	0.014051	0.008008	0.011209	0.002759
IV.	0.013797	0.020357	0.004427	0.003422	0.004552	0.002023
V.	0.012195	0.036444	0.015876	0.014093	0.011771	0.000743
VI.	0.016717	0.008770	0.017292	0.002878	0.003991	0.003875
VII.	0.022457	0.014181	0.025161	0.002707	0.009569	0.007986
Average	0.016355	0.017741	0.016067	0.004844	0.009630	0.005865

For this purpose it is necessary to calculate the values of the probabilities of the different judgments according to both hypotheses and form the differences between the observed and the calculated values. These deviations are squared and their sums formed. These results are given in Table VIII., which contains the sums of the squares of the deviations for each one of the seven subjects in our experiments on lifted weights. A mere glance at this table shows that the judgment as to the value of the two hypotheses in question cannot be doubtful for a minute. With the only exception of the psychometric function of the smaller judgments for subject V., all the sums of the squares of the deviations calculated by the  $\Phi(\gamma)$ -hypothesis are smaller than those calculated by the arctan-hypothesis. We,

<sup>1</sup> All the details of the calculation are given in the treatise on 'Die psychophysischen Massmethoden als Grundlagen empirischer Messungen,' *Archiv f. d. ges. Psychologie*, 1909, Vol. 16.

therefore, must say that the first hypothesis fits our results better than the second.

A closer examination of the data of Table VIII. shows that the agreement between the results of calculation and of observation is different not only for different subjects, but also for the different judgments. The averages of these sums for our seven subjects are 0.014767, 0.002829, 0.007325, 0.003332, 0.008869, 0.003581 and 0.006754. It is perhaps worth while noticing that subject II., who had by far the greatest practice in psychological experiments, has the smallest average, whereas subjects III. and V., who were the least reliable, have large averages. From this one might conclude that the psychometric functions approach the  $\phi(r)$ -type with increasing practice of the subject, but this conclusion does not agree with the fact that subjects I. and VII. have a large average, although their reliability manifests itself by a small coefficient of divergence. The averages of the sums of the squares of the deviations for the three judgments are given at the bottom of each column in Table VIII. The agreement between theory and observation is best for the smaller judgment, the second place being taken by the equality judgments, the third by the greater judgments. This result agrees very well with the standpoint we have taken before, that the psychometric functions may differ from individual to individual, and that the nature of the dependence of the probabilities on the intensity of the comparison stimulus may be different for the different judgments.

## A MARKED CASE OF MIMETIC IDEATION.<sup>1</sup>

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In the present discussion I wish to emphasize a point of view which I have set forth elsewhere<sup>2</sup> in regard to the nature of the mental image, namely that it is not always, perhaps not generally, due to centrally aroused processes alone, but that in part, at least, our imaginal experiences have mixed with them peripheral factors in such a way that it is impossible to separate these experiences out. This is particularly true of motor imagery. Indeed it may be doubted whether we ever experience a motor image in the sense of a purely cortical process. What is called a motor image may be in reality a motor sensation. Because of this difficulty of separating the centrally aroused processes from those peripherally aroused (especially if these latter are weak and indefinite), and because of the uncertainty attached to the term motor imagery, I have chosen the term motor ideation in the present paper, meaning to signify by this term those kinæsthetic experiences (whether central or peripheral in their origin) which we employ at times in our thinking and which constitute the mind-stuff of certain of our ideational processes.

It is common to divide ideational types into two main classes, namely, object types (*Sachvorstellungstypen*) and word types (*Wortvorstellungstypen*). Meumann, for example, enumerates under the former, beside the concrete visual, acoustic and tactile-motor types,— gustatory, olfactory and emotional types; further, motor types in which the ideational processes are in terms of imitative movements. Under the word types he enumerates the verbal-visual, the verbal-acoustic, and the verbal tactile-motor types.

<sup>1</sup> This paper was read before the American Psychological Association, at Cambridge, Mass., December 30, 1909.

<sup>2</sup> PSYCHOL. REV., 1908, XV., 158-168.



The writer of the present paper in introspecting concerning his own ideational processes, finds himself continually employing a kind of ideation that does not seem to be clearly brought out in this classification, nor to be emphasized to any extent in the literature. He is almost exclusively of a pure motor type in his ordinary thinking, although he does have the ability to revive at will concrete visual imagery of pronounced richness and variety; further, he has marked visual dream hallucinations and has at times experienced such hallucinations in his waking moments. Under certain circumstances he can mentally picture colors that give the impression of being more highly saturated than any he has ever seen in the spectrum. Notwithstanding this ability to recall sensory objects in such concrete ideational forms, in his ordinary thinking he only on rare occasions employs verbal-visual ideation. He does not see the printed or written word, and cannot readily tell from the visual appearance of a word whether it is properly spelled or not. He has no auditory ideation whatsoever, whether concrete or verbal, and so it happens that practically all of his thought processes are of the motor type.

With insignificant differences except for one important addition, his motor ideas are of the same character as those reported by Stricker<sup>1</sup> and by Dodge.<sup>2</sup> In his concrete motor ideation he finds like Stricker that when he recalls movements of his own body or of other persons or animals, he gets suggestions of muscular contraction in those parts of the body concerned in the movements. In following objects in motion or in recalling their movements, he, like Stricker, is conscious of a sensation or image of strain of the eye muscles. Beyond this, when he attempts to image certain objects that are at rest, as for example a circle, he finds that a part, perhaps all, of the visual idea is really kinæsthetic. He gets the clear impression in trying to recall the circumference of the circle, of eye movement in the orientation of the outlines of the figure, and of eye fixation when he turns his attention to the center. For this reason

<sup>1</sup> *Studien über die Sprachvorstellungen*, 1880, und *Studien über Bewegungsvorstellungen*, 1882.

<sup>2</sup> *Die motorischen Wortvorstellungen*, Halle, 1896.

he finds difficulty in recalling both circumference and center at the same time, and when he succeeds in doing this he is distinctly conscious of changing his type of ideation from motor to visual. In general he finds that his recall of the outlines of visible objects is distinctly a kinæsthetic and not a retinal affair.

When he introspects concerning his verbal ideation he finds that he is largely speech-motor in his thinking. He notices in general the same phenomena that are reported by Stricker and by Dodge in their respective accounts of their internal speech. There are the same rather definitely localized sensations from the lips, the tongue and the larynx as set forth by Stricker and amplified in detail by Dodge. In addition there is often the sensation of a rather diffused internal pressure at the base of the tongue, streaming upward toward the soft palate and with the distinct feeling of tension within the ear itself, accompanied, however, by not the slightest suggestion of a genuine auditory image. These sensations of pressure and tension are particularly marked when the writer is engaged in the silent reading of a foreign language, and have been observed at their maximum in learning nonsense syllables that are difficult to pronounce.

While the various ideational processes, thus briefly outlined, in a few minor particulars differ from what has already been reported concerning the extreme motor type, they do not, in the main, offer any facts strikingly new or significant. As has already been said, however, the writer finds in one particular, as yet not touched on, that his motor ideation is somewhat novel, and perhaps significant, in as much as he finds over and above what has already been definitely recorded in the literature a kind of motor thinking which may be termed, for lack of a better name, dramatic or mimetic. This dramatic or mimetic ideation is both concrete and symbolic. In its concrete form it may very well fall under the classification of Meumann, previously referred to under imitative movements in connection with concrete thought processes. If the writer recalls vividly such a situation as he sometimes has experienced on a still evening in the summer on the shores of Narragansett Bay, when the sand-flies have made life decidedly disagreeable, he is apt to reinstate

this situation in terms of a rapid movement (either actually executed or merely imagined) of his hand past his ear and face. In this case there is an actual revival of a situation in motor terms which is just as concrete and definite as if he had recalled in tactile terms this discomfort which the flies had caused him. There are many experiences of this sort where the image or the sensation of the movement becomes the vital aspect of the situation.

The novel aspect of this type of ideation, however, is not to be found in the reinstatement in a concrete way of these experiences, but in the symbolic use of this mimetic ideation, where the movements, either executed or merely recalled, are not definite reactions to a set of definite objective conditions, but rather become signs of such an objective reality, in much the same manner as words seen, heard or spoken are symbolic of the realities for which they stand. In this case the writer finds himself using not indeed an inner speech, but an inner sign language which carries for him the meaning often of abstract and colorless modes of thought. Just as words have become conventionalized so that their significance as actual processes of adjustment have in most instances been lost, so this mimetic ideation often does not apparently possess any marked significance as representing definite adjustments to a concrete situation, although at times it is expressive in a way in which words often are not. Apparently these mimetic ideas represent in part instinctive movements, and in part certain conventionalized gestures which have been acquired in individual experience, and which at one time may have had a concrete significance in adjustment, but which now have passed almost entirely from the concrete to the symbolic.

A few illustrations may make clear what the general nature of this symbolic mimetic ideation is. When the writer attempts to call to mind a series of words, a phrase, a paragraph, or even a collection of nonsense syllables, his first experience is not generally the recall of the words as such, but rather the emergence into consciousness of the background or setting of these words in terms of their general 'drift' or meaning. There seems to be a certain sort of rhythmical sequence in

which these words appear; what would be a spatial arrangement for one who thought in visual terms; not the spatial arrangement, however, of the words as they would appear on a printed page, but a spatial arrangement in terms of their meaning in which the transitional thought processes are represented by curved or zigzag movements; in which often there appears a sort of a plot with a distinct rise to a climax and perhaps a falling off at the end. This way of thinking comes out most clearly when the writer attempts to learn a series of nonsense syllables. If left to his own devices he finds himself invariably attempting to arrange these, even when he studies them in successive presentation, in a sort of a sequence of movements which seem to be essential to their subsequent recall. Unless he can recall in this way the general total movement he is often at a complete loss to revive the words that he has previously learned. When, however, he is able to reinstate the original background, then the words themselves come up readily and with comparatively little loss, even after a period of one or two days. On the other hand, if he attempts to learn a word series by mere *Einprägung* he finds such learning extremely slow and difficult, and he finds a corresponding difficulty in subsequent reinstatement.

The actual mind-stuff of this ideation he finds in sensations or images which represent gestures such as pointing, raising the index finger, curving the hand, and in more general kinæsthetic 'sets' which may involve the entire musculature. Such a sentence as this comes to the mind, 'Infinity broods over all things.' Immediately with the words themselves come into consciousness the speech-motor processes and further a general background of kinæsthetic symbolism. The kinæsthetic symbol for *infinity* is found in the tendency to prolong the word, this prolongation being accompanied by the distinct impression of projecting it from the mouth and then following this projected word by definite bodily movements. There is an image or sensation of a forcible and continued ejection by the speech-motor apparatus and of a bending forward and tension of the entire body, setting itself as if for flight. There is no visual symbol here, as for example, of extended space, or the

limitless vault of the heavens on a starlit night. The whole comes in motor adjustments. The word *broods* brings an entirely different suggestion. Here the ideation centers in a distinct picture (kinæsthetic not visual) of outstretched hands, and body bending forward and downward. *All* is symbolized by a sensation or image of roundness in the oval cavity, and by an extensive gesture (not actually executed, but merely represented) of an inclusive movement with both hands sweeping around and joining in front of the body. The symbol for *things* is the mental representation of a direct and sudden gesture with hand extended and index finger pointing out and downward.

It may be noticed from this description that much of this mimetic thinking is in the stage of what would correspond in the development of word-language to the period of ideographs in which the word-symbol suggests the concrete situation for which it stands. This is true particularly of the ideation in connection with the word *broods*. On the other hand *infinity* and *things* are represented by imaginal elements in which the concrete has been almost entirely replaced by the symbolic.

One further example may serve as an additional illustration of how this kinæsthetic ideation is employed. Recently two members of my beginning class in psychology reported to me that when they attempted to recall the expression *words seen*, they got beside the visual image of the words themselves, a picture of a curved line which for them symbolized the inflection of the voice in pronouncing these words. Here evidently was a case of visual symbolic imagery that formed a background to the words themselves. In the writer's case the whole would come in a purely kinæsthetic form. The words would be recalled solely in speech-motor terms, and the inflection of the voice in purely kinæsthetic ideation, either localized in the speech-organs themselves or represented by an imagined gesture (not seen but recalled in terms of motor adjustments) of the index finger describing the curve.

The writer's attention was first clearly called to his own excessive use of kinæsthetic ideation, when he attempted to set forth in an article in Woodbridge's journal<sup>1</sup> and later in a series

<sup>1</sup> *Journal of Philosophy, Psychology and Scientific Methods*, II., 229, 1905.

of discussions in the *Philosophical Review*<sup>1</sup> his standpoint concerning the noëtic intention. This involved analyzing out the mindstuff which formed the background of his experience in objectifying, in intending, in pointing. He then concluded that this intention had at its basis 'experienced sensations of muscular adjustment.'

Later in introspecting on his normal method of learning and recalling nonsense syllables, he became convinced that his kinæsthetic ideation, beyond and in excess of his speech-motor thinking, is the essential element in meaning for him. More and more he has become impressed with the fact that mimetic ideation is to a large extent the actual texture of his thought processes, and that while without careful analysis he might come to the conclusion which various psychologists have reached, that thought can flow on without an imaginal background, he is forced on making such an analysis to the conclusion that, although in his own thinking there is a marked lack of concrete or verbal ideation, he has a rich content of an imaginal nature in his mimetic imagery.

This belief was still further confirmed by a series of lectures delivered by Professor Titchener at the University of Illinois in March, 1909, and just issuing from the press. The subject of these lectures was the 'Experimental Psychology of the Thought Processes,' and the conclusion reached by the lecturer was in brief that it would not be surprising to discover "for minds of a certain constitution all conscious meaning is carried either by total kinæsthetic attitude or by words." The writer is convinced that his experience is a case in point.

While this type of ideation is probably not usual, I have reason to believe that it may be more common than may at first be supposed.<sup>2</sup> In a conversation with President G. Stanley Hall recently, I was informed by him that he possessed this type of thinking to a marked degree. He was kind enough to read to me an unpublished note describing his method of learning, when a boy, certain meaningless words and his method of re-

<sup>1</sup> 'The Intention of the Noëtic Psychosis,' XV., 307-311, 515-517, 1906.

<sup>2</sup> In the discussion which followed the presentation of this paper about one third of those present at the session reported that they possessed this type of ideation to a greater or less extent.

membering musical compositions. In his case the mimetic ideation is largely in imaged or partially executed dances and in other rhythmical movements that, however, are evidently in part concrete and in part symbolic adjustments to the actual situation.

I have also recently found still a third person who seems to be of this same general type; a person with marked motor ideation, with ineffectual visual ideation, and without auditory ideation; may it not be that there are many such cases, and is it not further likely that persons of a predominatingly visual or auditory type have a measure of this mimetic ideation: and may it not be that such persons are wrong in believing that their thought at times has no sensory or imaginal background, and that a more careful analysis would lead them to the discovery of just this thing which in a person of a pronounced motor type comes more noticeably to the foreground?

In conclusion, I wish to urge that the existence of this type of ideation is what we might naturally expect. The current biological theories in regard to consciousness, particularly the instrumentalism of Dewey, have made us familiar with the thought that the meaning of a situation is after all an attitude and that this attitude must be in the last analysis a motor affair. Judd has well expressed this in a recent article.<sup>1</sup> Here he says, "unity of percepts and unity of ideas are . . . phases which describe an aspect of consciousness dependent on motor tendencies."

It is but a step further to the conclusion that this general dependence of experience for its significance or motor adjustment has left a deposit of mind-stuff that symbolically represents concrete situations, not actually present, but thus ideally represented. This mind-stuff may in many instances have lost entirely its original significance and, as Baldwin has expressed it<sup>2</sup> in discussing motor attitudes expressive of emotions, may have resulted in apparent confusion 'due to the grinding, erosion, rivalry of development processes among themselves.' Thus those attitudes which once had a definite significance as attitudes

<sup>1</sup> *Journal of Philosophy, Psychology and Scientific Methods*, 'Motor Processes and Consciousness,' Feb. 18, 1909.

<sup>2</sup> *Mental Development*, third edition, p. 248, 1906.

having lost their specific meaning, are capable of taking on a symbolic character, thus functioning for meaning without themselves being as such concretely significant. In other words, the situation is no longer reinstated in terms of an actual, concrete adjustment, but rather in terms of a kinæsthetic symbol of that adjustment.



FROM THE UNIVERSITY OF CALIFORNIA  
PSYCHOLOGICAL LABORATORY.

XI. EXPERIMENTS ON THE REPRODUCTION OF DISTANCE AS  
INFLUENCED BY SUGGESTIONS OF ABILITY AND INABILITY.

BY GRACE MILDRED JONES, M.L.

Up to the present time the several experiments to determine the effect of suggestion have been made with some important differences in the method of investigation. In the earlier experiments the observer was given no instructions to resist any influence by suggestion; as, for example, in Small's<sup>1</sup> test, where a visual illusion was employed and the children given no warning of such; so in Binet's<sup>2</sup> experiment where the subjects were misinformed as to the true length of the lines in their relation to one another. The experiments of Pearce,<sup>3</sup> and of Smith and Sowton<sup>4</sup> were made under practically these same conditions.

With Brand<sup>5</sup> a radically new method was adopted. The subjects were aware of the purpose of the experiment and while the content of the suggestion was to be given place in the mind the observers were warned against any voluntary response to it. Furthermore, the reactions depended not alone upon visual perception but primarily upon the power to reproduce. Bell's<sup>6</sup> experiment in these essential conditions was identical with Brand's. It differed chiefly in this: that a visual type of suggestion was used in addition to the vocal.

In the present investigation, the method in general remained

<sup>1</sup> Small, 'The Suggestibility of Children,' *Pedagog. Sem.*, 1896, IV., pp. 176-220.

<sup>2</sup> Binet, La suggestibilité, *Annee Psych.* V., pp. 82-152.

<sup>3</sup> H. J. Pearce, 'Normal Motor Suggestibility,' *PSYCH. REV.*, 1902, IX., pp. 329-356.

<sup>4</sup> W. J. Smith and S. C. M. Sowton, 'Observations on Spatial Contrast and Confluence in Visual Perception,' *Brit. J. Psych.*, 1907, II., pp. 196-219.

<sup>5</sup> J. E. Brand, 'The Effect of Verbal Suggestion on the Estimation of Linear Magnitudes,' *PSYCH. REV.*, 1905, XII., pp. 41-49.

<sup>6</sup> J. C. Bell, 'The Effect of Suggestion upon the Reproduction of Triangles and Point Distances,' *American Journal of Psychology*, 1908, XIX., pp. 504-518.

the same as with Brand. The nature of the suggestions was changed and instead of being given in the form of a command they conveyed ideas of ability and inability. The chief difference lay in the fact that three types of suggestion were used, the vocal, the visual, and the 'auto,'<sup>1</sup> and that a special investigation was made of the relative effect of these various types.

#### METHOD OF EXPERIMENT.

The apparatus employed in the present experiment was almost identical with that used by Brand. At a distance of 80 centimeters from the subject two white pegs were set up showing through a narrow slit in a black screen; below this slit was another, somewhat wider, in which were exposed the visual suggestions. It was so arranged that the slits might be covered and uncovered conveniently to meet the needs of the experiment. At a distance of 40 centimeters from the observer was another black screen low enough so that the subject was able to see the horizontal slits in the farther screen where the pegs fixing the standard distance were exposed. On the nearer screen was a ledge where the subject was to adjust corresponding pegs in making his 'reproduction.' Both screens were built upon a table at a convenient level; the background was black and the room but dimly lighted. The pegs themselves were made clearly visible by screened lights.

The observer responded to six varieties of suggestion and to one signal where no suggestion was offered. Three types of suggestion were used—the visual, made by means of the printed mottoes "You are now able" and "You are now unable"; the vocal, made by the experimenter to the observer in the same words; and the 'auto,' made by the subject to himself in the words, "I am now able"; "I am now unable." The suggestions were given in irregular order; after each an interval of a second and a half was allowed before the exposure of the pegs, to give the subject time to concentrate his attention on the idea. The pegs were then exposed for a second and a half, the slide was replaced so as to conceal them, and the observer immediately

<sup>1</sup> 'Auto' is the name given to the type in which the observer responded to his own suggestion of ability or inability.

placed his pegs upon the ledge and adjusted them in accordance with his estimate of the distance between the exposed pegs. This distance, or space interval, was kept constant throughout the experiment; but of this the subjects were unaware, as their assertions prove. Each of the subjects took it for granted, when the absolute position of the pegs was changed, which occurred after every sixth judgment, that the space interval was changed also.

The experiment covered a period of five months, from October, 1908, to March, 1909; during this time the subjects were experimented upon at regular intervals and were required to give approximately the same number of judgments at each sitting. Three observers, experienced in psychological methods, were engaged. Five hundred twenty-five estimates were made by each of the three subjects; that is, seventy-five estimates for each variety of suggestion and seventy-five with no suggestion. The subject was instructed to allow each suggestion a place in his mind, to hold the idea it offered, but not to allow it consciously or intentionally to affect his estimate. Toward the close of the experiment each observer reported that he felt certain he was not being influenced in the least degree by any of the types of suggestion. According to their statements no feelings of ability or inability were aroused within them, nor was the experimenter able to observe any outward effect, such as hesitation or the like.

### RESULTS.

In that portion of Table I. which presents the averages for the combined seventy-five estimates the results are seen to be uniform for all the three subjects in these respects, that in all twenty-four 'groups'<sup>1</sup> where the affirmative of any type is compared with the negative of the same type, both the constant

<sup>1</sup> 'Group' is used here and throughout the account to mean a pair of averages, namely the affirmative and negative of any single type of suggestion or of the combined types—and this, either as regards average reproduction (or the constant error) or variability. For example, in Table I., in the first series of twenty-five judgments, 30.88 and 30.34 constitute a 'group' for subject X; 1.52 and 1.58 another group for the same subject. Thus for each series of twenty-five judgments there are eight groups for each subject, making twenty-four for the three subjects for that series.

error<sup>1</sup> and the variability<sup>2</sup> are appreciably less with the affirmative suggestions than with the corresponding negative suggestions. And again, in the averages obtained from judgments made with no suggestion there is always less constant error and usually less variability than in any of the averages of estimates with suggestion.

TABLE I.

THE AVERAGE REPRODUCTION OF THE STANDARD DISTANCE (30 CM.)

TOGETHER WITH THE VARIABILITY.

(The latter given in each case immediately below the average reproduction.)

Type of Suggestion.	Averages for First 25 Judgments. Subject			Averages for Second 25 Judgments. Subject			Averages for Third 25 Judgments. Subject			Averages for Combined 75 Judgments. Subject		
	X.	Y.	Z.	X.	Y.	Z.	X.	Y.	Z.	X.	Y.	Z.
Visual — affirmative	30.88	30.4	33.64	28.66	30.50	30.38	27.12	30.70	30.86	28.886	30.533	31.626
	1.52	1.64	3.68	1.74	.86	1.04	2.88	.94	1.42	2.046	1.146	2.046
Visual — negative	30.34	30.08	33.56	28.30	31.04	30.48	26.82	30.84	31.08	28.486	30.653	31.706
	1.58	2.04	3.72	1.9	1.48	1.36	3.18	1.08	1.56	2.22	1.533	2.213
Vocal — affirmative	30.94	30.06	33.18	29.16	30.64	30.06	27.75	30.68	31.06	29.28	30.46	31.433
	1.34	1.34	3.3	1.36	1.12	1.10	2.32	1.08	1.54	1.673	1.18	1.98
Vocal — negative	31.10	30.58	33.40	28.382	30.90	30.50	23.74	31.02	31.46	27.78	30.833	31.786
	1.9	1.66	3.64	1.7	1.1	1.54	2.26	1.38	1.62	1.953	1.38	2.266
Auto.—affirmative	30.72	30.06	32.96	28.46	30.48	30.16	27.74	30.88	31.76	28.973	30.473	31.626
	1.76	1.02	3.28	1.66	.84	.88	2.3	1.12	1.76	1.906	.993	1.973
Auto. — negative	30.88	30.28	32.88	28.54	30.50	30.70	27.40	30.74	31.48	28.94	30.506	31.686
	1.6	1.6	3.12	1.62	1.54	1.50	2.6	1.14	1.96	1.94	1.426	2.193
No suggestion	31.24	29.76	32.66	29.32	30.52	30.24	28.16	30.72	31.28	29.573	30.333	31.393
	1.72	1.72	2.74	1.24	.92	1.64	2.04	1.0	1.52	1.666	1.213	1.966
<sup>3</sup> All types of suggestion combined	30.81	30.243	33.27	28.583	30.676	30.38	26.761	30.81	31.283	28.724	30.576	31.643
	1.616	1.55	3.456	1.663	1.156	1.236	2.59	1.123	1.643	1.956	1.276	2.112
Affirmative suggestions combined	30.846	30.173	33.26	28.76	30.533	30.20	27.54	30.753	31.226	29.048	30.488	31.561
	1.54	1.333	3.42	1.56	.94	1.0	2.5	1.046	1.57	1.875	1.106	2.0
Negative suggestions combined	30.773	30.313	33.28	28.406	30.813	30.56	25.98	30.866	31.34	28.388	30.664	31.726
	1.693	1.766	3.49	1.74	1.373	1.46	2.68	1.2	1.71	2.037	1.446	2.224
Visual suggestions combined	30.61	30.24	33.60	28.48	30.77	30.43	26.96	30.77	30.97	28.686	30.593	31.66
	1.55	1.84	3.70	1.82	1.17	1.2	3.03	1.01	1.49	2.133	1.34	2.13
Vocal suggestions combined	31.02	30.32	33.29	28.77	30.77	30.28	25.75	30.85	31.26	28.513	30.646	31.61
	1.62	1.5	3.47	1.53	1.11	1.32	2.29	1.23	1.58	1.813	1.28	2.12
Auto. suggestions combined	30.80	30.17	32.92	28.50	30.49	30.43	27.57	30.81	31.62	28.956	30.49	31.65
	1.68	1.31	3.20	1.64	1.18	1.19	2.45	1.13	1.86	1.923	1.21	2.083

<sup>1</sup>The 'constant error' is the difference between the standard distance (30 cm.) and the 'average reproduction'—the latter being the average of the actual estimates made. Thus the 'average reproduction' for the three estimates 33, 28 and 31 would be 30.67; the 'constant error' 0.67.

<sup>2</sup>'Variability' (=the 'crude variable error') was obtained by adding the amount of variation from the standard (30 cm.), regardless of its sign, and dividing by the number of cases considered; thus the 'variability' for the three judgments 33, 28 and 31 would be 2.

<sup>3</sup>"All types of suggestion combined" excludes throughout estimates with no suggestion.

TABLE II.

AS REGARDS THE AVERAGE REPRODUCTIONS.

1. *Average Reproductions Compared with the Actual Distance Exposed.*

Subject.	First 25 Judgments.	Second 25 Judgments.	Third 25 Judgments.	Entire 75 Judgments.
X	Reprod. Dist. >	Reprod. Dist. <	Reprod. Dist. <	Reprod. Dist. <
Y	" " > (except 'no suggestion')	" " >	" " >	" " >
Z	Reprod. Dist. >	" " >	" " >	" " >

2. *Average Reproductions with no Suggestion Compared with Average Reproductions with Suggestion of any Kind.*

X	no sug. >	no sug. <	no sug. <	no sug. <
Y	" " < (except 3 affirm. cases and visual -)	" " < (except visual + and auto. + and -)	" " < (except visual + and vocal +)	" " <
Z	no sug. <	no sug. < (except vocal + auto. +)	no sug. < (except visual + vocal +)	" " <

3. *Average Reproductions with Affirmative Suggestion Compared with Average Reproduction with Negative Suggestion.*

X	vis. + > vis. - voc. + < voc. - auto. + < auto. -	vis. + < vis. - voc. + < voc. - auto. + > auto. -	vis. + < vis. - voc. + < voc. - auto. + < auto. -	vis. + < vis. - voc. + < voc. - auto. + < auto. -
Y	vis. + > vis. - voc. + < voc. - auto. + < auto. -	vis. + < vis. - voc. + < voc. - auto. + < auto. -	vis. + < vis. - voc. + < voc. - auto. + < auto. -	vis. + < vis. - voc. + < voc. - auto. + < auto. -
Z	vis. + > vis. - voc. + < voc. - auto. + > auto. -	vis. + < vis. - voc. + < voc. - auto. + < auto. -	vis. + < vis. - voc. + < voc. - auto. + > auto. -	vis. + < vis. - voc. + < voc. - auto. + < auto. -

AS REGARDS VARIABILITY.

4. *Variability when there was no Suggestion Compared with that when there was Suggestion of any kind.*

Subject.	First 25 Judgments.	Second 25 Judgments.	Third 25 Judgments.	Entire 75 Judgments.
X	no sug. > (except vocal - and auto. +)	no sug. <	no sug. <	no sug. <
Y	no sug. >	no sug. < (except visual + and auto. +)	no sug. < (except visual +)	no sug. < (except visual +, vocal +, auto +)
Z	" " <	no sug. >	no sug. < (except visual +)	no sug. <

TABLE II. *Continued.*

5. *Variability when there was Affirmative Suggestion Compared with that when there was Negative Suggestion.*

X	vis. + < vis. — voc. + < voc. — auto. + > auto. —	vis. + < vis. — voc. + < voc. — auto. + > auto. —	vis. + < vis. — voc. + > voc. — auto. + < auto. —	vis. + < vis. — voc. + < voc. — auto. + < auto. —
Y	vis. + < vis. — voc. + < voc. — auto. + < auto. —	vis. + < vis. — voc. + < voc. — auto. + < auto. —	vis. + < vis. — voc. + < voc. — auto. + < auto. —	vis. + < vis. — voc. + < voc. — auto. + < auto. —
Z	vis. + < vis. — voc. + < voc. — auto. + < auto. —	vis. + < vis. — voc. + < voc. — auto. + < auto. —	vis. + < vis. — voc. + < voc. — auto. + < auto. —	vis. + < vis. — voc. + < voc. — auto. + < auto. —

In the portion of Table I. which presents the averages for the three successive series of twenty-five judgments, as we might expect, this general contrast in the effect of affirmative and negative suggestions appears somewhat less regularly; yet the general result of combining the entire 75 judgments under any one type of suggestion is seen to be fairly well distributed over these smaller series.

Thus in the averages for the first series of twenty-five estimates seven out of the twenty-four groups showed a reversed effect for the affirmative and negative suggestions, that is, the negative had less average error or variability than the affirmative; five of these irregularities were as regards the error, the other two as regards the variability. In the second series of twenty-five estimates three of the twenty-four groups were irregular, one in respect to the error, the other two in respect to the variability. In the third series of twenty-five estimates, three irregularities out of the twenty-four groups again occurred, two as regards the error and one as regards the variability.

Accordingly, out of the seventy-two groups, when the judgments were considered in series of twenty-five, there were thirteen irregular groups. Of these thirteen irregularities seven were with the 'auto' type of suggestion, three were with the visual, two with the vocal, and one appeared in the comparison of 'affirmative suggestions combined' and 'negative suggestions combined,' in the first twenty-five estimates.

As regards the smaller constant error in the estimates made with no suggestion than with those where there was suggestion,

the results apparent in the combined seventy-five judgments appear also, in each of the series of twenty-five judgments, save for subject X in the first series. As regards the smaller variability with no suggestion, exceptions occur in both the first and second series of twenty-five judgments, but none in the third series.

In view of the fact that the judgments made under suggestion whether affirmative or negative show so frequently an increase in variability and error beyond that in the judgments made without suggestion, we may infer that suggestion does in itself, and apart from the actual 'contents' of the suggestion, effect some change in the reproduction of distance.

But it is also significant that the suggestion acts to a considerable extent in a direction corresponding to the actual 'contents' of the suggestion given; *i. e.*, the error and variability under suggestions of *ability* were almost always less than when suggestions of *inability* were made.

Previous experimenters found that one subject differed from another radically in the degree and nature of the change which suggestion produced. This is true only within somewhat narrow limits, in the present experiments. The three subjects showed the same significant tendencies in responding to suggestion; that is, with each, the suggestions of ability produced generally less error and less variability than did the suggestions of inability; and estimates with no suggestion were in each case still nearer the standard. The only difference lay in the fact that with two of the subjects there was a constant tendency to lengthen, with the other subject to shorten, the reproductions of the standard interval of 30 cm. And this constant error, whatever its direction, was increased by suggestions, especially by those of 'inability.' It would be unjustifiable to say that one observer showed more susceptibility to suggestion than another merely upon the ground of a different direction or a different absolute amount in his departure from the standard. Rather the subjects should be compared upon a basis of the *change* in the amount or direction of the errors or variability according as suggestions were present or absent, or according as one form of suggestion or another was employed. Upon this basis (see Table III.) there appears some difference between the three subjects.

## TABLE III.

## ORDER OF THE EFFECTIVENESS OF SUGGESTION.

Based upon seventy-five judgments for each type of suggestion.

*I. As Measured by the Change in the Average Reproduction.*

(The types of suggestion decrease in effectiveness from left to right.)

Subject.

<i>X</i>	Voc. —	Vis. —	Vis. +	Auto. —	Auto. +	Voc. +	No suggestion
<i>Y</i>	Voc. —	Vis. —	Vis. +	Auto. —	Auto. +	Voc. +	No suggestion
<i>Z</i>	Voc. —	Vis. —	Auto. —	Auto. +	Vis. +	Voc. +	No suggestion

*II. As Measured by the Change in Variability.*

(The types of suggestion decrease in effectiveness from left to right.)

Subject.

<i>X</i>	Vis. —	Vis. +	Voc. —	Auto. —	Auto. +	Voc. +	No suggestion
<i>Y</i>	Vis. —	Auto. —	Voc. —	No sug.	Voc. +	Vis. +	Auto. +
<i>Z</i>	Voc. —	Vis. —	Auto. —	Vis. +	Voc. +	Auto. +	No suggestion

Bell in his experiment notes that the effect of suggestion decreased with repetition. This seems to be only partly, if at all, true here. If the fact that suggestions of ability produce less error, and suggestions of inability greater error in the reproduction of distance attests anything as to the power of suggestion this experiment shows that the susceptibility more regularly occurs with repetition, since as the experiment proceeds there occur fewer groups where this effect is reversed. Table I. shows seven irregularities in this respect in the first series of twenty-five estimates, and three each in the second and the third series of twenty-five estimates. Apart from irregularity, the absolute amount of the difference between 'affirmative suggestions combined' and 'negative suggestions combined' shows in the case of Subject *X* a uniform *increase* in the successive series of twenty-five judgments; and the same is true of the difference between 'no suggestion' and 'all types of suggestion combined.' With the other subjects there is neither a uniform increase nor decrease.

The purpose of the experiment was only in part to investigate the effect of suggestion in general; to determine the relative influence of the different *types* of suggestion was equally the purpose.

In Tables III. and IV. is set forth the relative strength or effectiveness of these different types of suggestion. It there appears that for each of the subjects the negative suggestions



TABLE IV.

Type of Suggestions.	Ratios of Reproductions with Suggestion to Reproductions Without. Based upon the Constant Error in Reproduction and also upon the Variability; Latter in Parentheses.			Ratios of Reproductions with Suggestion to the Actual Standard (30 cm.). Based upon the Error in Reproduction.		
	Subject			Subject		
	X.	Y.	Z.	X.	Y.	Z.
All types of suggestion combined. (450 reproductions)	.971 (1.174)	1.008 (1.051)	1.007 (1.074)	.957	1.019	1.054
Affirmative suggestions combined. (225 reproductions)	.982 (1.125)	1.005 (.911)	1.005 (1.017)	.968	1.016	1.052
Negative suggestions combined. (225 reproductions)	.959 (1.222)	1.010 (1.192)	1.010 (1.131)	.946	1.022	1.057
Visual suggestions combined. (150 reproductions)	.970 (1.280)	1.008 (1.104)	1.008 (1.083)	.956	1.019	1.055
Vocal suggestions combined. (150 reproductions)	.964 (1.088)	1.010 (1.055)	1.006 (1.078)	.950	1.021	1.053
Auto. suggestions combined. (150 reproductions)	.979 (1.154)	1.005 (.997)	1.008 (1.059)	.965	1.016	1.055

(suggestions of inability) were about twice as effective as were the positive suggestions (suggestions of ability). Moreover for each subject the vocal negative suggestions were the most effective of all as measured by the constant change in the average reproduction. As measured by the variability, the visual negative suggestions were stronger. With all the subjects the vocal positive suggestions, on the other hand, had the least influence upon the average reproduction; while upon the variability the 'auto' positive type was among those having the weakest influence.

As suggestions of ability and inability appeared to bring about an increase in the constant error and in the variability the question was raised as to whether the results were due merely to the *distraction* which the suggestions produced, and not at all to their inherent 'content' or ideas. It hardly seemed that such a theory could be fully justified, for it would not account for the fact that different types of suggestion had regularly produced different effects — effects consistent with the 'content' of the suggestion. However, seventy-five additional tests were made on each of the three subjects, in which the pegs were exposed and the reproductions made while the observer counted aloud the strokes of a metronome, swinging at five different rates, in order that the counting might more likely act as a dis-

traction and not become a merely mechanical operation. The other conditions were identical with those in the tests made for suggestibility.

TABLE V.

RESULTS FOR ESTIMATES MADE UNDER DISTRACTION.

Subject.	Average Reproductions of the Standard Distance together with the Variability.			
	Averages for First 25 Judgments.	Averages for Second 25 Judgments.	Averages for Third 25 Judgments.	Averages for Com- bined 75 Judgments.
<i>X</i>	28.48	28.32	28.12	28.31
	1.68	1.64	1.88	1.73
<i>Y</i>	29.86	29.38	29.15	29.48
	.94	.86	.93	.89
<i>Z</i>	29.18	29.14	31.65	29.92
	1.02	1.54	1.88	1.46

The variability is thus seen to be, on the average, less than when suggestions were offered, and for the most of the subjects less even than in the cases where no suggestion was made. As for the average reproductions, the two subjects whose estimate of the distance had been constantly greater than the standard in the previous experiment now had a tendency to make it less than the standard. The other subject whose tendency had been to underestimate the standard interval now had a tendency to make it still shorter.

The fact that the results with distraction differed in these respects from the results with different types of suggestion indicates that at least with two of the three subjects the peculiar effect of these suggestions is not due solely nor even predominately to any distraction which may have inhered in them.

## XII. THE EFFECT OF VARIOUS TYPES OF SUGGESTION UPON MUSCULAR ACTIVITY.

BY EDW. K. STRONG, JR., M.S.

The purpose of the experiment here reported was to discover how far and in what manner maximum muscular activity may be affected by suggestions of various types. In all these the subject was instructed not to oppose any resistance to the suggestion nor on the other hand consciously to endeavor to carry out the suggestions, but his attention and conscious effort throughout was to be expended in exerting each time his maximum grip. Yet he was requested to maintain the suggestion in consciousness until after his muscular effort.

### HISTORICAL REVIEW.

There is scarcely any literature which bears directly upon the question under discussion in this paper. A number of articles<sup>1</sup> which have appeared lately indicate that some attention is being given the subject, but neither their methods nor their results have any direct relation to those of the present investigation. As far as I am aware Brand is the pioneer in this field of investigation. His experiment<sup>2</sup> was performed in this laboratory, and his purpose, as he states it, was "to find out how far and in what direction the visual estimation of a linear magnitude could be affected by suggestion of certain possible errors in such estimation, the subject knowing that the

<sup>1</sup> Triplet, 'Dynamogenic Factors in Pace-making and Competition,' *Amer. Jour. of Psych.*, IX., 507-533. Small, 'The Suggestibility of Children,' *Ped. Sem.*, IV., 182. Binet, 'La suggestibilité,' *L'année psych.*, V., 99. Pearce, 'Normal Motor Suggestibility,' *Psych. Rev.*, IX., 348. Smith and Sowton, 'Observations on Spatial Contrast and Confluence in Visual Perception,' *Brit. Jour. Psych.*, II., 196-219. A very full discussion of all but the first accompanies the article of Bell ('The Effect of Suggestion upon the Reproduction of Triangles and of Point Distances') in the *Amer. Jour. of Psych.*, XIX., 504.

<sup>2</sup> 'The Effect of Verbal Suggestion upon the Estimation of Linear Magnitudes,' *Psych. Rev.*, XII., 1905, 41-49.

suggestions were purely arbitrary, *i. e.*, that they had no reference to any foreseen tendency to err in any direction.”<sup>1</sup>

The eight different suggestions used by Brand in his experiment were printed upon white cardboard in letters 1.2 cm. high. The conductor presented them to the subject by displaying the cardboard for a moment. After the suggestion had been presented the conductor displayed two small objects upon his frame and then called upon the subject to respond by setting up his two similar objects upon his own frame at a distance from each other approximating as nearly as possible that of the original objects.

I have reduced the results as given in his tables to percentages of the respective standards.

The following table gives the total results of Subject ‘C,’ eliminating the first group of experiments, since their variation is more than ten per cent. from the standard, due probably to lack of practice.

TABLE I.

Suggestion.	No. of Judgments.	Sum of Standards.	Result in per cent.
Zwp fjvic bgzx asye.....	49	1,258	100.5
Life is real where.....	49	1,258	99.5
Don't make too long.....	49	1,258	101.0
Don't make too short.....	49	1,258	101.0
Make short .....	104	2,632	99.5
Make long.....	104	2,632	102.0

And the following table gives the total results for Subject ‘Y,’ the first group of data having been likewise eliminated.

TABLE II.

Suggestion.	No. of Judgments.	Sum of Standards.	Result in per cent.
Zwp fjvic bgzx asye.....	49	1,086	91.0
Life is real where.....	49	1,086	90.0
Don't make too long.....	84	1,810	92.0
Don't make too short.....	84	1,810	92.5
Make short.....	100	2,400	94.0
Make long.....	100	2,400	94.0

<sup>1</sup> It is just at this point that suggestions as experimented upon in Bell's (see page 5) and Brand's work and in my own differ from the others. As Bell puts it: "The most potent factor without doubt in cases of suggestion is the arousal of an attitude of general expectancy." This factor has been utilized in the experiments of others, but is so far as possible eliminated in these three, for here the subject was particularly instructed that he was not to respond consciously to the suggestion but to endeavor consciously to exert his maximum grip.

From these tables I do not feel that any clear-cut deductions can be made, with the possible exception that the two brief suggestions 'Make short' and 'Make long' tend more than the other suggestions to make the reproduced distance greater in magnitude, and the two suggestions 'Don't make too long' and 'Don't make too short' tend to a less degree to have the same effect.

In Table III. Brand has brought together from group D (or the last group of experiments with both subjects) the totals for all the suggestions containing the word 'long' in one column and all those containing the word 'short' in another column, for comparison. The percentages as given below were inserted by myself.

TABLE III.

Subject.	No. of Judgments.	Sum of Standards.	'Long.'	Per cent.	'Short.'	Per cent.
'C'	58	1,628	1,702.3	104.5	1,644.4	101.0
'Y'	36	580	556.3	95.5	542.6	93.5

If compiled from all four of the groups instead of group D alone we have the following.

TABLE IV.

Subject.	No. of Judgments.	Sum of Standards.	'Long.'	Per cent.	'Short.'	Per cent.
'C'	207	4,932	5,095.8	103.0	5,052.2	102.5
'Y'	189	4,294	4,008.2	93.8	4,013.2	93.9

Here we have no superiority of the 'long' mottoes over the 'short' mottoes, except with the one subject.

Let us now turn to the second part of his experiment. Here but three suggestions were used: 'long,' 'short' and 'XXXX.' The latter was used as a check or neutral. Quoting from him: "Four subjects were employed and the results were not very uniform, two of the subjects showing no decided tendency towards anything resembling a constant effect, while the other two subjects showed a clear general constancy of considerable difference throughout."

In conclusion it seems to me that one cannot point with any emphasis to any direct effect from the above varied suggestions upon estimation of distance except (1) that 'long' suggestions may consistently affect one person more than corresponding

'short' ones; (2) that the reverse effect may be produced upon a second person; and (3) that there may be no appreciable effect of one or the other upon a third person.

In Bell's experiment<sup>1</sup> two types of suggestion were used — the auditory and the visual. For the former such as 'make high,' 'make low,' 'make high enough,' etc., or simply 'high' and 'low' were used, being spoken by the conductor just before the presentation of the object to be reacted upon. For the visual suggestion a diamond-shaped figure 20 cm. long and 4 cm. wide was shown to the subject. When displayed in a vertical position it was thought that it might serve as a 'high' suggestion, being much taller than any other figure shown. Likewise when shown in a horizontal position it was called a 'low' suggestion. (As far as I am aware the subject was not informed what this visual suggestion was intended to suggest. It may not then have had an equal effect upon the different subjects.)

The forms chosen for reproduction were: (1) triangles of different shapes and heights and (2) vertical point distances as shown by (a) a dot above the center of a base line and (b) a dot above another dot.

Under the first group (triangles of different shapes and heights) ten triangles were used, of the same base (10 mm.), but of different heights and shapes, and varying from 49 to 100 mm. in altitude. The work was so carried on that nine reproductions of each triangle were made with each kind of suggestion (*i. e.*, auditory and visual). Three of the nine were reproduced with 'high' suggestions, three with 'low' suggestions, and three without any suggestion. The altitude of the reproduced triangle was compared with the standard, and the difference expressed in millimeters as a positive or negative error. But the weakness of his experiment lies in the small number of observations per subject per triangle per suggestion, *i. e.*, three in number. Averages drawn from so few observations are hardly of greatest weight.

Bell concludes "that in general the suggestions do affect the reproduction of the triangles; that the auditory suggestion

<sup>1</sup> 'The Effect of Suggestion upon the Reproduction of Triangles and of Point Distances,' *Amer. Jour. of Psych.*, XIX., 1908, 504.

is more effective than the visual; and that in the auditory set the 'low' suggestion is more effective than the 'high.' " Upon looking at the subjects individually we find they have reacted differently. Subject 'B' alone showed striking susceptibility to the suggestion in all the cases. Subjects 'A,' 'C' and 'D' with the auditory suggestions and Subject 'E' with the visual suggestions, estimated the apex of the triangles below the standard when given either 'high' or 'low' suggestions. Throughout the experiment susceptibility to 'low' suggestions was more general and more uniform than to 'high' suggestions.

Under the second group (vertical point distances) the dot-above-line experiment was completed with auditory suggestions and the dot-above-dot experiment with visual suggestions. Bell in commenting upon his results states that there was little indication that the suggestions had any constant effect: with subjects 'C' and 'D' the 'high' and 'low' suggestions are both lower than the standard, the 'low' the lowest; while with subjects 'E' and 'F' the 'high' and 'low' suggestions are both higher than the standard.

#### DESCRIPTION OF THE PRESENT EXPERIMENT.

The series of experiments carried on as described below can be divided into two parts, the first part consisting of those experiments which were performed during the months from September to December, 1908, and the second part consisting of those which were performed during the months from January to April, 1909.

The general plan of the experiment was to give the subject a suggestion, and then have him respond each time with his maximum grip. Collin's elliptical form of dynamometer was used and from it an expression in kilograms was obtained of his muscular activity.

The subject was seated in a chair which was provided with two flat arms about two and a half inches wide which extended as far forward as the front edge of the seat. The subject rested his arms from the elbow to the wrist upon these arms of the chair. When actually gripping the dynamometer the palm of his hand was uppermost. In the intervals he was allowed to

rest his hand as he chose. The conductor sat at a small table. On the edge of this table between the subject and the conductor a large upright black screen was placed in such a manner that the subject could not see anything upon the table; nor could he see the conductor.

Once every twenty seconds, until the series with one hand was completed, the subject gripped the dynamometer with his maximum force. The exact procedure was as follows. When the second-hand of the watch indicated the moment of presentation, the suggestion was given by the conductor. The dynamometer was then placed in the subject's hand. As soon as the latter had responded with his maximum grip the conductor took the instrument from his hand, noted the reading, and then awaited the next moment of presentation.

In all there were seven suggestions offered. They might be classified as follows:

I. *Auditory Suggestions.*

1. Positive. "Now you can make it stronger than usual."
2. Negative. "Now you can't make it as strong as usual."

These suggestions were presented vocally by the conductor.

II. *Visual Suggestions.*

1. Positive. A plus (+) sign 0.4 inches in size displayed upon a piece of cardboard two inches square.
2. Negative. A minus (—) sign 0.4 inches in size displayed upon a piece of cardboard two inches square.

In giving these suggestions the conductor placed the cardboard upon the corner of the table in front of the subject.

At the commencement of the experiment each subject was told that the plus (+) sign was meant to suggest to him that he could make his grip stronger than usual and the minus (—) sign was meant to suggest that he could not make it as strong as usual. These signs were consequently visual suggestions depending on previous vocal instruction.

III. *Auto-suggestions.* Here the conductor announced in the same tone and manner as in presenting the Auditory Suggestions "Now you can make a suggestion of your own." The subject understood by this that he was at liberty to suggest to himself either the positive or negative suggestion and to designate



his choice to the conductor by audibly announcing it. In this case as soon as the subject had announced his suggestion the dynamometer was handed him and the experiments continued as usual.

1. Positive. (After suggestion by the conductor, as above.) "Now I can make it stronger than usual," spoken by the subject.

2. Negative. (After suggestion by the conductor, as above.) "Now I can't make it as strong as usual," spoken by the subject.

IV. *Neutral 'Suggestion.'* This consisted of the announcement by the conductor of, 'Now, neutral,' and was intended to act merely as a check and guide to what would be the exertion if no suggestion of any sort were given.

In the earlier experiments a series consisted of fifty-six experiments or readings, twenty-eight with each hand. These twenty-eight in turn consisted of four each of the seven different suggestions. These twenty-eight were presented to the subject in a haphazard arrangement previously determined upon. All subjects were given the same haphazard arrangements and in the same order, so that direct comparisons between them could be made. The right hand in every case was experimented upon first. After four days' work with each subject, ten neutrals instead of four were introduced, thus making a day's work or series consist of thirty-four experiments instead of twenty-eight. Besides these twenty-eight or thirty-four experiments there were always two extra neutrals at the start which were for the purpose of practice and were always discarded.

A day's work with subject 'B' consisted of two series during one hour,—that is to say, of fifty-six experiments with each hand taken in this manner: twenty-eight with the right hand, then twenty-eight with the left hand, then twenty-eight with the right, and finally twenty-eight more with the left. A day's work with subjects 'J' and 'W' consisted of only one series during one hour. During the second term but one series per day was taken with any one of the three subjects. It should also be stated here that all three subjects were familiar with experimental work.

The procedure during the *second term* was exactly the same as in the first term except for the following. Instead of seven suggestions nine were employed. The two auto-suggestions were omitted and the following four added. Two auditory suggestions consisting of 'Now, plus' and 'Now, minus' to correspond to the two visual suggestions employed during the first term. Also two visual suggestions consisting of the mottoes: "Now you can make it stronger than usual" and "Now you can't make it as strong as usual" printed on cards  $5.5 \times 1.25$  inches. These visual suggestions were to correspond to the auditory suggestions employed during the first term. Moreover the visual suggestions (+) and (—) were presented on cards similar to those just described, instead of on cards two inches square. Instead of 28 or 34 experiments on each hand per day there were 42 such experiments. These 42 consisted of four each of the eight suggestions and ten of the neutrals, arranged as before in a haphazard manner. During the second term the right hand was not experimented upon first each day, but only in alternation with the left hand.

The nine suggestions of the second term and the two auto-suggestions of the first term will be referred to in the tables by the following set of symbols.

I. *Auditory Suggestions* — in quotation marks.

'Can' means "Now you can make it stronger than usual."

'Can't' means "Now you can't make it as strong as usual."

'+' means "Now plus."

'—' means "Now minus."

II. *Visual Suggestions* — in brackets.

(Can) means the motto, "Now you can make it stronger than usual" printed on a card.

(Can't) means the motto, "Now you can't make it as strong as usual" printed on a card.

(+) means the symbol '+' printed on a card.

(—) means the symbol '—' printed on a card.

III. *Auto-suggestions*.

+ Auto means "Now I can make it stronger than usual," spoken by the subject.

— Auto means "Now I can't make it as strong as usual," spoken by the subject.

IV. *Neutral 'Suggestion.'*

'N' means "Now, neutral."

## THE RESULTS.

In the following two tables there are presented the averages of 48 experiments for each type of suggestion with either hand of the three subjects, the numbers indicating in kilograms the maximal grip.

TABLE V.  
RESULTS FOR THE FIRST TERM.

The numbers for the first six suggestions under Subjects 'B' and 'J' are an average of 48 results, and under Subject 'W' are an average of 24 results. The neutrals under Subjects 'B' and 'J' are an average of 96 results while under Subject 'W' they are an average of 36 results.

Suggestions.	Subject 'B.'		Subject 'J.'		Subject 'W.'	
	Right.	Left.	Right.	Left.	Right.	Left.
'Can'	38.8	29.6	17.2	15.0	33.1	23.7
'Can't'	38.4	28.9	17.1	15.4	33.3	23.4
(+)	38.9	29.9	17.7	15.0	33.8	24.4
(-)	38.6	29.3	17.1	14.8	33.5	24.4
+ Auto.	39.7	29.5	17.5	15.2	33.5	24.2
- Auto.	39.0	29.9	17.1	15.3	33.6	25.6
Average	38.9	29.5	17.2	15.1	33.5	24.3
Neutral	39.0	29.4	16.9	15.0	33.4	23.5
Average of all	38.9	29.5	17.1	15.1	33.5	23.9

TABLE VI.  
RESULTS FOR THE SECOND TERM.

The numbers for the first eight suggestions are an average of 48 trials while those for the neutrals are an average of 120 trials.

Suggestions.	Subject 'B.'		Subject 'J.'		Subject 'W.'	
	Right.	Left.	Right.	Left.	Right.	Left.
'Can'	40.3	31.8	16.9	15.9	32.8	25.1
'Can't'	39.7	31.4	16.9	16.3	32.6	25.4
(Can)	39.4	31.0	16.9	16.4	32.9	25.3
(Can't)	40.1	31.3	16.8	15.9	33.2	25.3
(+)	39.6	31.2	16.7	16.2	32.2	24.8
(-)	39.4	30.8	16.7	16.1	32.1	25.1
(+)	39.3	31.3	16.9	16.3	32.4	25.3
(-)	39.6	31.3	16.7	16.4	32.8	25.9
Average	39.7	31.3	16.8	16.2	32.6	25.3
Neutral	39.4	30.8	16.7	16.0	32.2	24.9
Average of all	39.5	31.1	16.8	16.1	32.5	25.2

From these two tables we see very clearly that suggestion as a whole heightens the maxima. In every case, except for the right hand of subject 'B' during the first term, the average of the neutrals for any of the three subjects with either hand is less than the corresponding average of all the other suggestions. The exception can probably be explained by an introspection of subject 'B' on March 1, when he stated that neutrals at *first* had a positive effect, that is to say, he felt that then was a time to make a 'record.'

In the following two tables we have a statement of the mean variation of the quantities entering into the preceding tables: Table VII stating the mean variation of the quantities in Table V, and Table VIII stating those of Table VI.

MEAN VARIATION TABLE VII. (FIRST TERM.)

Suggestions.	Subject 'B.'		Subject 'J.'		Subject 'W.'	
	Right.	Left.	Right.	Left.	Right.	Left.
'Can'	1.88	2.00	.86	.96	3.17	1.68
'Can't'	2.34	1.70	.74	.87	2.77	1.48
(+)	2.59	1.89	1.11	1.09	2.46	1.71
(-)	2.29	1.84	1.04	.83	2.62	1.70
+ Auto.	2.21	2.32	.93	.81	2.30	1.61
- Auto.	1.74	1.86	.93	.88	2.26	1.98
Average	2.17	1.93	.93	.91	2.59	1.69
Neutral	1.94	1.57	.85	.94	2.99	2.08
Average of all	2.14	1.88	.92	.91	2.65	1.75

MEAN VARIATION TABLE VIII. (SECOND TERM.)

Suggestions.	Subject 'B'		Subject 'J'		Subject 'W'	
	Right.	Left.	Right.	Left.	Right.	Left.
'Can'	2.58	1.54	.63	1.03	2.17	2.32
'Can't'	2.57	1.88	.73	1.01	2.62	2.31
(Can)	2.04	.80	.76	.91	2.06	2.19
(Can't)	1.80	.67	.67	1.02	2.38	2.39
+	1.82	.80	.79	1.08	2.32	2.24
-	2.17	.80	.77	1.13	2.61	1.97
(+)	2.46	.87	.81	1.20	2.11	2.04
(-)	1.97	.92	.89	1.13	2.11	2.06
Average	2.18	1.03	.76	1.06	2.29	2.19
Neutral	2.05	1.69	.85	1.12	2.37	2.50
Average of all	2.16	1.11	.77	1.07	2.31	2.22

In Table IX. we have a restatement of Table V., and in Table X. restatement of Table VI. to show the effect respectively of positive, negative and neutral suggestions upon the three subjects.

From these tables it is evident that the negative suggestions tend more than the positive suggestions with subject 'W' to heighten the maxima, and this is especially true with his left hand. But with the other two subjects the positive suggestions as a general rule are superior to the negative in this respect. However in all three cases the negative auto-suggestions with the left hand are clearly superior to the positive in heightening the maxima. Why this is the case is difficult to say. Possibly, the subject feels that after audibly declaring that he can't make it as strong as usual, he must exert greater effort in order to neutralize the suggestion's effect and actually obtain his maximum. With the right hand, however, this tendency does not appear, perhaps because of the hand's greater use and its consequent greater fineness in discrimination. Indeed it appears

TABLE IX.

SUBJECT 'B.'

	With the Right Hand.			With the Left Hand.		
	Positive.	Negative.	Neutral.	Positive.	Negative.	Neutral.
Auditory	38.8	38.4		29.6	28.9	
Visual	38.9	38.6		29.9	29.3	
Auto.	39.7	39.0		29.5	29.9	
Neutral			39.0			29.4
Average	39.1	38.7	39.0	29.7	29.4	29.4

SUBJECT 'J.'

Auditory	17.2	17.1		15.0	15.4	
Visual	17.1	17.1		15.0	14.8	
Auto.	17.5	17.1		15.2	15.3	
Neutral			16.9			15.0
Average	17.3	17.1	16.9	15.1	15.2	15.0

SUBJECT 'W.'

Auditory	33.1	33.3		23.7	23.4	
Visual	33.8	33.5		24.4	24.4	
Auto.	33.5	33.6		24.2	25.6	
Neutral			33.4			23.5
Average	33.5	33.5	33.4	24.1	24.5	23.5

TABLE X.  
SUBJECT 'B.'

	With the Right Hand.			With the Left Hand.		
	Positive.	Negative.	Neutral.	Positive.	Negative.	Neutral.
Auditory can-can't	40.3	39.7		31.8	31.4	
Visual can-can't	39.4	40.1		31.0	31.3	
Auditory + and —	39.6	39.4		31.2	30.8	
Visual + and —	39.3	39.6		31.3	31.3	
Neutral			39.4			30.8
Average	39.7	39.7	39.4	31.3	31.2	30.8

SUBJECT 'J.'

Auditory can-can't	16.9	16.9		15.9	16.3	
Visual can-can't	16.9	16.8		16.4	15.9	
Auditory + and —	16.7	16.7		16.2	16.1	
Visual + and —	16.9	16.7		16.3	16.4	
Neutral			16.7			16.0
Average	16.9	16.8	16.7	16.2	16.1	16.0

SUBJECT 'W.'

Auditory can-can't	32.8	32.6		25.1	25.4	
Visual can-can't	32.9	33.2		25.3	25.3	
Auditory + and —	32.2	32.1		24.8	25.1	
Visual + and —	32.4	32.8		25.3	25.9	
Neutral			32.2			24.9
Average	32.6	32.7	32.2	25.1	25.5	24.9

true that the so-called maximum effort in this experiment is not a real maximum exertion each time, but rather is an effort to attain a sort of definite standard. The left hand is thus at a disadvantage in maintaining its maximum, or definite standard, because of its lesser use and inferior discrimination.

In Tables XI. and XII. we have still a different restatement of Tables V. and VI. for the purpose of showing the effect of particular types of suggestion. In these tables the figure placed opposite each suggestion is the average of that type's positive and negative suggestions taken together; and the types are arranged according to the magnitudes of the averages.

We must conclude from these tables that: (1) the auto-suggestions tend most strongly of all the types of suggestion to heighten the maxima; (2) that during the first term the visual suggestions were superior in this respect to the auditory suggestions with subjects 'B' and 'W,' and were inferior with subject

TABLE XI.  
WITH THE RIGHT HAND.

Subject 'B.'	Subject 'J.'	Subject 'W.'
Auto. 39.35	Auto. 17.3	Visual 33.65
Neutral 39.0	Auditory 17.15	Auto. 33.55
Visual 38.75	Visual 17.1	Neutral 33.4
Auditory 38.6	Neutral 16.9	Auditory 33.2

WITH THE LEFT HAND.

Auto. 29.7	Auto. 15.25	Auto. 24.9
Visual 29.6	Auditory 15.2	Visual 24.4
Neutral 29.4	Neutral 15.0	Auditory 23.55
Auditory 29.25	Visual 14.9	Neutral 23.5

TABLE XII.  
WITH THE RIGHT HAND.

Subject 'B.'	Subject 'J.'	Subject 'W.'
Auditory Can-Can't 40.0	Auditory Can-Can't 16.9	Visual Can-Can't 33.1
Visual Can-Can't 39.8	Visual Can-Can't 16.9	Auditory Can-Can't 32.7
Auditory + and — 39.5	Auditory + and — 16.8	Visual + and — 32.6
Visual + and — 39.4	Visual + and — 16.8	Neutral 32.2
Neutral 39.4	Neutral 16.7	Auditory + and — 32.1

WITH THE LEFT HAND.

Auditory Can-Can't 31.6	Visual + and — 16.4	Visual + and — 25.6
Visual + and — 31.3	Visual Can-Can't 16.2	Visual Can-Can't 25.3
Visual Can-Can't 31.2	Auditory + and — 16.2	Auditory Can-Can't 25.2
Auditory + and — 31.0	Auditory Can-Can't 16.1	Auditory + and — 25.0
Neutral 30.8	Neutral 16.0	Neutral 24.9

'J,' while during the second term the visual suggestions were superior with subject 'W' and the left hand of subject 'J', and were inferior with the right hand of subjects 'B' and 'J'; and (3) that during the second term the motto suggestions (*e. g.*, "Now you can make it stronger than usual") were superior to the symbol suggestions (*e. g.*, plus sign) in heightening the maxima.

Before attempting any explanation of these results it should be borne in mind that Miss G. M. Jones, working at the same general problem outlined in this paper, and with two of the same subjects, but upon the reproduction of distance instead of upon maximal exertion, obtains the most accurate reproduction of distance with the neutral 'suggestion,' while the other suggestions,

instead of aiding in the reproduction of distance, apparently disturb the accuracy of such reproduction. (It is notable that all three subjects were affected nearly alike in her experiment while there were such different effects with different subjects in my own experiment, as well as in Brand's.)

Suggestion then affects the reproduction of distance by acting as a disturbing factor, but aids, as my results show, when applied to maximal muscular effort. From these two experiments it seems probable that when *accurate* work is to be done all suggestions prevent the best work; but when mere *amount* of muscular effort is called for, any arousal of the attention to the work acts as a stimulant and enables the person to do better than he would otherwise have done. This effect is also shown by an incident which occurred with subject 'W.' Near the close of the series with his left hand the experiments were interrupted by a messenger who notified him that he was awaited elsewhere. The eagerness to be through the experiment caused a rise of approximately five kilograms, or eleven pounds, in his grip for the remaining eight trials.

I have used the word 'attention' in this connection because I do not know a better, yet I am not entirely satisfied that it is a question of the attention, *i. e.*, that it is an intellectual arousal that causes the results described in this paper. That there is an arousal of the whole person is certain; but whether the heightened maxima, as in the case just cited, are due to the attention directly or to an indirect effect of the general stir, it is difficult to be certain. I do not feel that it is a question of the will primarily, for that factor is supposed to be eliminated from the experiment by the instruction at the commencement of work, when the subject was told to make his maximum effort each time.

Yet with these misgivings, it seems natural to explain the greater efficacy of the auto-suggestions by a greater concentration of the attention upon the work in hand. An auto-suggestion consisted of the statement by the conductor of the experiment, "Now you can make a suggestion of your own"; this was followed by the statement of the subject, "Now I can make it stronger than usual" or "Now I can't make it as strong as



usual." The whole process tended to call forth greater concentration of attention to the next grip of the dynamometer than would result from either the auditory or visual suggestion, where there was only an act on the part of the conductor.

On the other hand a certain broad arousal perhaps accounts for the marked superiority of the motto suggestions, "Now you can make it stronger than usual" and "Now you can't make it as strong as usual" over the symbol suggestions, '+ ' and '-.' The latter, especially when spoken ('auditory suggestion') occupied but a moment and did not arouse the subject as did the motto-suggestions, for the latter required a greater length of time to be understood.

Introspections of all three subjects were in general that the suggestions had little effect upon them. For example at the end of the second term subject 'B' once stated that the auditory suggestion 'can' seemed perhaps most powerful and the visual suggestion (+) was next, but that it did not seem as if any were strong enough to produce an effect. And about the same time subject 'J' stated her feeling as follows, "When I stop to think the suggestion seems to have little effect, but when not thinking I feel that I obey the suggestion." Throughout the experiment notes were taken of those cases in which the subject expressed himself as satisfied or dissatisfied with the result. A careful analysis of these fails to show any correlation between the actual result and the introspections.

### XIII. THE LOCALIZATION OF DIASCLEROTIC LIGHT.

BY G. M. STRATTON.

Some time ago Veraguth<sup>1</sup> announced that the sensation produced by stimulating the retina through the sclerotic coat instead of through the pupil is often localized upon the same side as that upon which the stimulus falls, and that this is particularly true when the stimulus falls upon the temporal side. With some persons, however, he found that under these circumstances there was, in addition to the sensation on the temporal side, a weaker sensation localized upon the nasal side. On the other hand, when the stimulus was applied to the nasal side the sensation was always localized in the normal way, that is, it was referred to the temporal side. When the light was applied to the sclerotic, not in a radial, or diametric, direction, but in a direction approximating that of the tangent at that point, no change was noticed in the place the light seemed to occupy in the visual field. From this fact Veraguth argues against the assumption that the light, when it falls upon the temporal side of the sclerotic, passes through and strikes the retina upon the nasal side and therefore is referred in quite normal fashion to the side opposite to that upon which it really affects the retina. He believes that we have here an indubitable departure from the common cross-localization of visual impressions, and would explain this departure by the biological principle of utility. As regards the need of a correct localization of light penetrating the sclerotic, there is, he feels, a great difference between the temporal side of the eye which is open and exposed, and the inner or nasal side which is screened by the nose. And he maintains that the correct orientation of light coming diasclerotically is important only when the light falls on the *temporal* side; on the nasal side the light would come not only through the sclerotic coat, but through the pupil, and therefore would

<sup>1</sup> 'Die Verlegung diaskleral in das menschliche Auge einfallender Lichtreize in den Raum,' *Zeitschrift für Psychologie*, Vol. 42, pp. 162 ff.

be localized correctly according to the well-known law. Pressure phosphenes, unlike many of these sensations produced by diasclerotic stimulation, are all projected, he finds, to the opposite side of the visual field; and this to him seems reasonable, because there is no advantage in their being projected otherwise. They therefore follow the general law for the projection of a stimulus which comes through the pupil.

Veraguth believes that the present phenomena are a stumbling block in the way of the nativistic theory of vision which supposes that the space value of the impression inheres in the retinal element. For, if this were the case, why should there be a difference between the localization of the sensation resulting from the diasclerotic stimulation and that from the diapupillary? But the empiristic theory, he argues, can well admit and explain these diverse facts, for it supposes that localization is due to a complex of factors, of which the retinal factor is but one. Now the diasclerotic and the diapupillary stimulation, he holds, may each arouse a different group of factors to determine its localization. Each may well be connected, for example, with a different group of muscular responses, and therefore, according to the empiristic view, have a different localization. With some persons it is not improbable that both the complex of factors concerned in diasclerotic localization and the complex concerned in diapupillary localization may come into play simultaneously, and thus there be brought to pass by diasclerotic stimulation a localization at once on both sides of the visual field. Thus he would explain the double projection which was sometimes noticed in his experiments.

To control Veraguth's data and explanation a number of experiments were tried by the present writer, at first by means of a very strong beam of artificial light in a dark-room and brought to a point on the sclerotic coat by means of a system of screens and lenses. Afterwards, a device essentially the same as that employed by Veraguth himself<sup>1</sup> was adopted. It consisted of a portable flash-light so covered at the end that the light was emitted only from a circular area less than one millimeter in diameter.

<sup>1</sup> Veraguth, "Zur Prüfung der Lichtreaction der Pupillen," *Neurologisches Centralblatt* (16 April, 1905), XXIV. Jahrg., 338 ff.

This small opening lying well forward and beyond the body of the lamp could be brought close to the sclerotic coat of the eye, yet without actual contact. In this way the disturbing sensations, as well as the reflexes so difficult to suppress, were avoided. The observations were made by four persons; by two of these during frequent repetitions of the experiment over a considerable stretch of time. In general, these experiments while confirming much that Veraguth himself reports, yet bring some modification and supplement of his data, and lead, I venture to suggest, to a different conclusion.

There are regions upon the temporal side where the light is localized in the normal way; that is, upon the nasal side. There are regions where the light is localized entirely upon the temporal side. There is often found also a zone where there is some kind of localization upon both sides at once. The relation of these zones to one another is as follows: The region where the sensations are localized on the same side begins immediately posterior to the outer edge of the iris and extends backward a distance nearly equal to the distance from the center of the pupil to the outer edge of the iris. The region where the localization is upon the opposite side is still farther back. The region of double localization lies at the junction, or between the borders, of these two zones.

Upon the nasal side of the eye my own observations confirm, but not entirely, those of Veraguth. The localization is now far more frequently upon the side opposite to that upon which the stimulus falls, than in the case of temporal stimulation through the sclerotic coat. Yet occasionally there are found localities well forward where the sensation is localized upon the same side as the stimulus.

The localization of pressure phosphenes shows this peculiarity: that the phosphenes, so far as I can observe, are obtainable only upon those regions of the eye where light stimulus, passing through the sclerotic coat, arouses what I should call a more definite, a more figurate or punctiform sensation. In the anterior region where the application of light upon the sclerotic gives a vague sensation of light, local pressure upon the sclerotic coat produces no phosphenes whatever. The phosphenes,

however, when produced, are always localized upon the side opposite to that of the incidence of the stimulus, as Veraguth himself observed. By test experiments in which there was carefully noted the angle in the visual field where the extreme outlying phosphenes are localized, I find them ceasing to appear at about the outmost limit for the perception of light coming through the pupil. This would tend to confirm one in the belief that the retinal limit for response to actual light is the same as that for pressure stimulation.

Veraguth has laid considerable emphasis upon the fact that no change in the localization of the sensation takes place when, instead of having the stimulus come to the outer coat of the eye in a diametric direction, it is introduced in a direction approximately that of the tangent at that point. It does not seem, however, that this fact should be regarded as of great importance. It *would* be of importance were the outer coats of the eye perfectly transparent; but the sclerotic, being translucent, would in any event diffuse the light much as would tissue-paper or egg-shell; that is to say, the light would be transmitted in all directions from the point of incidence more or less indifferently, whether the light itself, in arriving at that point, came in one direction or another. In consequence, we should expect that if the sensation itself were in anywise due to the passage of light into the bulb of the eye, and over to the retina upon the opposite side, it would not be affected by a change from diametric to tangential incidence.

Another fact which must be taken into account in the explanation of the experimental data is that there is a retinal zone of considerable width anterior to the *ora serrata* which contains neither rods nor cones.<sup>1</sup> Yet this is a region where light, falling upon the sclerotic coat, nevertheless produces light sensations. And, furthermore, the present experiments lead me to believe that this is the region especially liable to the production of the diffused sensations which are localized upon the temporal side, both by temporal and by nasal stimulation. It would seem, to

<sup>1</sup>See Piersol, *Human Anatomy*, 1907, pp. 1456, 1467; Tolot, *Anatomischer Atlas*, 1907, p. 892; Werner Spalteholz, *Hand Atlas of Human Anatomy*, tr. Barker, III., 772, 780; Huber, *Text-book of Histology*, tr. Cushing, 1900, p. 422. I am indebted to my colleague, Dr. Moody, for assistance upon this point

express it mildly, anomalous to explain the localization of light here as though there were actual light-sensitive elements directly beneath the region of the sclerotic where the stimulus falls.

There is, one must confess, some difficulty at the present time in giving any explanation of all the facts which appear in this interesting experiment. Yet on the whole there would seem sufficient reason to believe that Veraguth's theory is forced and improbable.

In the first place, it is hardly in keeping with other well-known facts of retinal behavior to suppose that the stimulation of the very same retinal elements will lead to such different localization by reason of some change in the manner of *approach* to those elements. The stimulation of the rods and cones by pressure, for example,—a stimulation which also comes through the sclerotic, and consequently by a course entirely different from that of ordinary photic stimulation—occasions no upset of the normal localization of these elements; their sensation is referred to exactly the same place as though it were caused by light, and had come through the pupil.

The facts perhaps are better explained by assuming that the light which penetrates the sclerotic coat *posterior* to the *ora serrata* and which consequently excites the light-sensitive elements of the retina directly beneath or adjacent produces the definite or figurate sensation which is localized upon the opposite side. This is the region where the localization of phosphenes and that of sensations produced by light stimulus are identical. But where the light falls *anterior* to the *ora serrata* it of course cannot reach immediately and upon the same side of the eye light-sensitive elements but can stimulate them only after first being diffused through the interior of the bulb. The character of this diffusion will be such that the light will pass in straight lines from its point of incidence upon the sclerotic, but in lines traversing the interior of the bulb in *all* directions and not just in the direction which is a continuation of that along which it was travelling before its incidence. The behavior of the light here is such as one would get in a dark room if a beam of light fell upon a plate of ground-glass in an aperture of the

wall. There would be a general illumination of the room but naturally more intense upon the side opposite to the illuminated spot, yet diffused and unfigurate upon that side.

This would account for two facts: namely, that the sensation in the case of such light stimulation as falls very far forward on the sclerotic is exceedingly indefinite, and is localized upon the same side as that upon which the light itself has been introduced into the eye. Furthermore, it would account for the fact, already referred to, that a change in the direction along which the light falls upon the sclerotic does not alter the character of the projection, provided the point of incidence remain unchanged. The position of greatest illumination in our dark-room would not be altered perceptibly by altering the direction along which the light proceeded to the plate of ground-glass.

The behavior of the sensation produced by light falling on the nasal side one must confess is mildly puzzling. For the most part all stimuli here, no matter how far forward they fall, are referred to the opposite side; yet we should expect that if we go forward of the *ora serrata* on this side we should find that the sensations were referred exclusively to the nasal side. The facts here obtained may perhaps be accounted for by the fact that the *ora serrata* upon this side reaches farther forward<sup>1</sup> than upon the opposite side, in accord with the familiar observation that the field of view upon the temporal side is always wider than upon the nasal. Consequently the stimulus introduced diasclerotically upon the nasal side would almost inevitably find light-sensitive elements adjacent to, if not directly beneath, the point of incidence; and the sensation thereby produced would by its intensity quite drown any sensation arising from a diffusion inside the eye. It is possible also that this phenomenon of irregular localization is to some extent occasioned by a reflection of light from the surface of the crystalline lens perhaps near its periphery. This surface of the lens might easily reflect the light back to the same side as that upon which it entered the sclerotic, though to a point somewhat farther back. Thus it would reach the sensitive portions of the retina and now quite normally be localized upon the opposite side.

<sup>1</sup> See, *e. g.*, Werner Spalteholtz, *op. cit.*, III., p. 780.

All cases where there is a double localization from a single stimulation, that is to say, a localization at once upon both sides of the visual field, could well be accounted for by supposing that the light, after passing through the sclerotic, reaches not solely those light-sensitive elements which lie in the portions of the retina immediately beneath or adjacent, but is also diffused to the opposite side of the inner chamber and there falling diffusely upon the retina is referred to the side opposite to that upon which the more definite sensation is localized. We should thus have two spacially disjoined sensations occurring from a single impression because the stimulus actually fell upon disjoined portions of the retina.

My own feeling therefore is that nothing appears in these experiments with diasclerotic light which is at all discordant with the law governing the localization of diapupillary impressions. Visual localization is based upon the positions which objects normally occupy when stimulating the different points of the retina; and this localization, once established, takes no account whatever of the course by which the stimulus happens actually to arrive. No questions are asked as to whether it has entered by the door or has broken in as a thief and a robber; it is treated in all cases psychically alike.



# THE PSYCHOLOGICAL REVIEW.

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## A UNIT-CONCEPT OF CONSCIOUSNESS.

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Psychology lacks a serviceable unit-concept of mind, that is, a cautious reduction of consciousness to the lowest terms compatible with the limitations of science. Vain efforts to sound the psychical depths that lie beyond the range of verifiable knowledge, we have had in abundance. The sleeping monads of Leibnitz, the soul-cells and cell-souls of Haeckel, the psyche that is not necessarily conscious, of Verworn, are some of the attempts to rationalize the regions of infra-consciousness, sub-consciousness, or mere sentience. But these names mean the same thing if indeed they may be said to mean anything; they all suggest the same *tertium quid*, that bridge of cobweb, closed to science, spanning the chasm between conscious mind and insensate matter.

We are in need of a concept with more practical utility, one that will set bounds for comparative psychology by indicating how far downward in the scale of organic beings its surveys properly extend, also that will embody in itself the demonstrable antecedents from which the human mind has evolved. It is instructive to note how other sciences have profited by appropriate concepts of this sort and intention. Biology adopted the living cell, and wherever the cell is found the biologist has a legitimate field for his labor. Every physical science has analyzed its own subject-matter into simplest parts, each of which is conceived as similar in many important respects to every other. The purposes of the biologist did not require that the cell be divided into any lesser homogeneous units; but the physicist has made a further reduction of matter to molecules,

lying far beyond the range of the senses. The molecule is, consequently, rather a mental construct than a material object. The chemical atom at present illustrates the way a unit-concept may be altered to serve the purposes of advancing knowledge. Ions and particles are its outgrowths. For mechanics, there is the material particle, devoid of dimensions but possessing weight; and for geometry, the mathematical point, wholly immaterial but possessing position.

It is an accident that all these physical units have reference to matter and occupy space. Psychical units are equally possible, as is attested by the psychosis and the mental elements in descriptive psychology, the term in logic, the voluntary act in ethics, the family in sociology, and the like-minded group in the science of history. The sole qualification of every such unit is that it embody an auxiliary concept rendering a particular subject-matter more congruous and systematic. Psychology, treated as a natural science, needs some unit-concept that will systemize our notions of mind in organisms; the concept should involve a description of a certain type of mind, the simplest having the virtue of manifesting itself through bodily behavior so that its presence may become evident to the investigator. However, in psychology such a construction has never been attained, nor even scarcely attempted. It is a curious paradox, this case of a science seemingly in philosophic doubt concerning the reality of its own subject-matter. We defer to biology in treating lowly organisms as reflex machines, although this view places consciousness beyond the reach of investigation; and we borrow from physiology the unit-concept of the 'reflex-arc,' the peculiar feature of which is the absence of consciousness in all its operations.

If comparative psychology is not a possible science, by all means let us hasten to acknowledge the fact; if it is a possible science, much blame for its present condition belongs to psychologists themselves. Looking at the past, the whole body of psychological theory is seen to suffer by an overvaluation of the intellect at the expense of the other faculties; the result is a one-sided development of the science of mind. The study of sensation has been pursued so vigorously because, through the

senses, the mind is brought into relation with the physical world, and thereby the basis for intellectual development is laid. We have neglected almost completely the study of the feelings, which are the reactions of the mind itself upon receipt of these messages in the form of sensations. The formation of a serviceable unit-concept demands a better balanced treatment of the two sides of the problem, a fair consideration of the claims both of sensation and feeling. Unless this attitude is adopted, our conception of consciousness will be that of a mirror in which external reality is merely reflected, and a mirror-consciousness will never give external evidence of its own existence.

By calling attention to certain fictitious types of mind that have no dynamic relation to physical events, and thus have no significance whatsoever for science, we may acquire by contrast an idea of those absent features that might make such a mind an efficient factor and give it a 'survival value' in the process of organic evolution. In general, no mind composed either wholly of feelings or wholly of sensations could be made the object of scientific investigation.

For, in the first place, a creature without sensations could at best have only a series of panoramic visions, mere states of feeling in which to dream away its existence.<sup>1</sup> Secondly, a creature without feelings, though capable of sensation, could not be proved to exist. Indeed, no valid reason could be offered why it should be conscious of its sensations. If it had no subjective feelings, it would be indifferent to its sensations, in which case mere physical stimuli, acting on its body without arousing consciousness, would just as well serve to adjust it to its environment.<sup>2</sup> Again, by combining these two types in certain ways, we arrive at a third conceivable type of do-nothing consciousness, and also a fourth. For we may imagine the two groups

<sup>1</sup> This type is pure 'Gedankendichtung.' Dr. Holmes's 'Chambered Nautilus' is perhaps the best representative of the class. This organism had no inkling of the vital operations going forward in the webs of living gauze that formed its prison-house. But to the group belongs also a large assortment of sleeping monads, cell-souls, psyches, and psychoids.

<sup>2</sup> To this type belong all conceptions built on the analogy of the brainless frog, and the nerve-muscle preparation; it is schematically represented by the reflex-arc.

— the sensations and the feelings — as occupying, so to speak, two separate compartments. Then virtually two do-nothing minds might dwell in the same body and remain as indeterminate as either one would be if alone. Lastly, we may choose to imagine the two groups as connected in a pair-formation, like the order in which the animals came out of the ark — each particular sensation being on all occasions attended by its appropriate and invariable feeling, a strict monogamous relation. This last conception deserves careful consideration. It has been a more serious stumbling-block to psychology than one at first glance is apt to realize.<sup>1</sup>

To all of these types one judgment pertains: they are hypothetical, more hypothetical than is any disembodied spirit. If one should capture a disembodied spirit it ought wholly to suffice us; if, on the contrary, one should detect, somewhere in space, a do-nothing spirit of any of the above-described types, there would remain a second task, viz., that of establishing its transcendental connection with the particular organism that houses it. In what sense such a consciousness could *belong* to its own body would surely require an exceptional metaphysician to explain.

A historical survey of the doctrines reared upon these barren conceptions would not profit us in the present task. So numerous and influential have such views been however, that they have retarded psychology in its growth. Particularly undesirable are the doctrines advocated by many physiologists and biologists, which state that consciousness of every sort, even that of man himself, is of a do-nothing type. They have fostered the belief that mind has no causal relation toward matter and motion, consequently that no dynamic concept of mind can be formed. Condillac, by postulating sensation as the sole source of mental growth, rounded out a description that involved every factor of human consciousness. His doctrine of sensationalism went through its cruder stages to high degrees of refinement; but external stimuli were always taken as the only causes of sensation; sensations, the only causes of mind; the mind, there-

<sup>1</sup> This type supplies the mechanism for Descartes's theory of brute minds, for Condillac's marble statue, and for Clifford's automaton.

fore, could originate nothing, for it was an unalloyed product of its environment. Plainly this view did not transcend the limits of mirror-consciousness. And where is the flaw in the thesis that renders the conception impracticable for psychology, if psychology is at all possible? I think we find it in the fundamental assumption, that feelings are attributes of sensations: we are asked to suppose that when a particular sensation arises in consciousness, its proper feeling is bound to appear with it; the two are inseparable and the feeling is subordinate in that its specific nature is determined by the accompanying sensation as its immediate cause or occasion.

Out of this sensationalism developed the doctrine of automatism — merely by a refinement in terminology and a clearer vision of the implications involved. To the automatist, consciousness is an epiphenomenon, as ineffectual in the control of the bodily mechanism of any organism as the whistle of the locomotive is ineffectual over the running of the wheels. Yielding assent to this view with the scientist's besetting frailty for a paradox, we can give account of the origin of the manuscript of Hamlet, erasures and all, "without in the slightest degree acknowledging the existence of the thoughts in Shakespeare's mind."<sup>1</sup> With Judd I believe that all psychologists in years to come will regard this belief as "one of the curious fallacies of immature science."<sup>2</sup> Nevertheless, twenty years ago, no one could have formulated a theory of the feelings that would hold its own in debate against the clear and simple statements of the automatists. So feebly were their assertions disputed that Wundt, who has since led the psychologists out of this Egypt of materialism, was, in 1893, still treating the feelings of pleasantness and unpleasantness as attributes (Gefühlstöne) of sensations.<sup>3</sup> The exodus has since grown general, until, of all the notable leaders of the science, Professor Stumpf now stands

<sup>1</sup> James's illustration in *The Principles of Psychology*, Vol. I., v, 132.

<sup>2</sup> Judd, *Psychology, General Introduction*, 1907, p. 62. Cf. also Judd: 'Evolution and Consciousness,' *PSYCH. REV.*, March, 1910, 77-97. An interesting sidelight on Judd's view of the function of consciousness is provided by F. A. Woods in an article, 'Laws of Diminishing Environmental Influences,' *Popular Science Monthly*, April, 1910, 313-337.

<sup>3</sup> Wundt, *Physiol. Psychol.*, 4te Aufl., 1893, Bd. I., Cap. x. Cf. also Titchener, *Psychology of Feeling and Attention*, 1908, Lecture IV., 125.

virtually alone as the champion of the position that pleasantness and unpleasantness are sensational.<sup>1</sup>

A new doctrine of the feelings has been forming, but it is far from completion, and uniformity of opinion is not yet attained. The change of attitude is seen in the treatment of sensuous feelings, no longer as attributes of sensations, but as owing their origin each to a great number of causes of which the sensation is merely one. A feeling is considered to be the resultant of all the factors present in consciousness at the time of its origin — of memory images, masses of organic sensations, lingering traces of previous feelings, *et cætera* — and when attention is particularly directed toward any incoming sensation, the prevailing feeling becomes 'sensuous' by becoming simpler and by having more direct reference to the sensation concerned. What, on this basis, a sensuous feeling would be like in an exceedingly simple sensorium, we are still unable to say, but at least its origin is to be attributed to some slight network of causes in the guise of contemporary sensations and lingering feeling, together forming a thin stream of consciousness.

In the light of this new doctrine, the cause-and-effect relation between the physical world and consciousness assumes a more complicated form. The living creature, in respect to its modes of behavior, can scarcely any longer be treated as a reflex machine, a mechanism completely controlled from without; it is apt to be an organism, autonomous, governed from within. The point at issue involves a nice distinction: we need not accord to the organism any freedom of will, nor need we enter into metaphysical problems at all. We are called to judge whether the laws of consciousness are inherently identical with those of external nature, in which case the phenomena of the two worlds form a single nexus; or whether these two bodies of law, however consistent, are different in that nothing else in nature conforms to the same government as does the mind. The latter alternative leads to the conclusion that consciousness is a unique and effective factor in the evolution of things; also that

<sup>1</sup> Cf. M. W. Calkins, 'The Abandonment of Sensationalism in Psychology,' *Amer. Jour. of Psychol.*, XX., 1909, 269. For a very able criticism of Stumpf's position, see Titchener, *op. cit.*, Lecture III., 81.

the mind should be accorded a 'survival value' in our scientific explanations.

If the mind accords to any unique law of its own, it is not probable that this law pertains directly to sensation; rather it is operative in the subjective group of mental phenomena, the feelings. We should make a division therefore on this basis. After assigning the body, and in particular the nervous system, to the proper place as a part of the physical environment of the mind, we should note that all sensations are results of external stimuli that directly produce certain physiological events in the brain; consequently, the sensations, *causally considered*, belong on the physical side in that they are direct results of what we are to call physical. On the other side lies the uncharted territory of the feelings. Present knowledge is very meager concerning them, but possibly enough is already known to point the way toward a serviceable unit-concept, since this quest involves feeling only in its most rudimentary forms.

Taking this view of the matter, we should try to keep the proposed concept as free from objectionable implications as may be, for this precaution will contribute to the general utility of the result. There seems no better way to accomplish this than by a strict observance of Occam's 'principle of parsimony'—not to make more assumptions than are necessary. It is like erecting a barrier around the unit-concept while it is in process of construction. The organism is not to possess any innate information concerning its environment, and the environment is not to wait outside, ready made, and eager for an opportunity to make itself better known to the mind pent up within the body; in other words, we shall abjure teleological aids. Also, inside the barrier, it is necessary to clear the ground of unverifiable notions, such as subconsciousness and certain 'powers of mind,' original intuitions of space and time, and the innate desire for pleasure and avoidance of pain. There should, of course, be no talk of complex mental states, ideas or percepts, no presuppositions about a conscious memory; nothing but sensations and feelings forming a somewhat consecutive flow on the surface of consciousness.

But no matter how cautiously we prepare for the attempt,

the proposal will meet with objections. Some will predict that we shall find ourselves restricted to a barren sort of structural psychology according to which the mind from the inside will appear formal and static, without the power to make itself move. Sufficient justification seems given by the fact that we are trying to form a working hypothesis, the merits of which must be determined by the character of the deductions that may be drawn from it in the field of functional psychology, that is, through the study of animal behavior. Another objection is more serious. It will be said that other minds are unknowable, that we cannot participate in any consciousness other than our own, and that the most primitive consciousness is the most inscrutable and unknowable of all. This is the usual objection offered by those whose main interest lies with the natural sciences. So cautious is Driesch on this score that in speaking of conscious organisms he adopts the term 'psychoid'—“that is, a something which though not a 'psyche' can only be described in terms analogous to those of psychology.” And he also remarks that “the words 'soul,' 'mind' or 'psyche' present themselves, but one of them would lead us into what we have so carefully avoided all along, viz., pseudo-psychology.”<sup>1</sup>

The view-point of Driesch is external to mind, and for biological theory perhaps it should be. The concept of a psychoid is a convenient though artificial means for limiting his universe of discourse. But why psychologists should adopt the same view is beyond rational explanation, for it means that a vast deal of time has already been wasted in developing a pseudo-science, and also that whatever they may contribute to it is but the outpouring of other psychoids and to be regarded and treated as such.

Even physical science does not allow the adoption of this agnostic attitude unreservedly. Many of its unit-concepts are

<sup>1</sup> Driesch, *The Science and Philosophy of the Organism* (Gifford Lectures, 1908), p. 82. Cf. *idem*, p. 53: “By no means, of course, do we intend by our appeal to psychology to introduce that sort of pseudo-psychology which we excluded from natural science when we were studying instincts. All acting organisms, including acting men, are to us simply *natural bodies in motion*: at least they are *immediately* presented to us as such. . . . These agents or factors, however, would by no means be psychological in the introspective sense—the only sense which the word 'psychological' may legitimately possess.”



unknowable in exactly the same way as primitive consciousness is. Indeed the physical atom and the primitive mind are equally unknowable and in exactly the same sense,—unknowable as to what they are, but knowable as to what they do. Physicists use the concepts of power, energy, strain, without knowing anything about their intrinsic nature apart from what they are as states of human consciousness. Psychologists in fact do not leap as far when they attribute a ‘feeling of strain’ or a ‘sensation of contact’ to a primitive mind. But when a feeling of strain is postulated as an experience of a primitive mind, we do not mean that we can describe it. It is in that sense quite unknowable, and if it could be transferred into our own immediate experience we might not recognize it as similar to any of our customary experiences. As a postulate, it is formal, merely a typical conscious state standing in a definite relation to the initiation of bodily movement, the same relation as is borne by feelings of strain in human consciousness.

Disclaiming all intention of describing concrete experiences as felt by lower organisms, let us proceed to a closer consideration of the three necessary assumptions already adopted, namely, consciousness, sensations, and feelings.

1. *Consciousness*. — A sensation, entering consciousness, as surely meets with an environment and reacts to it as does an organism when it enters upon its physical environment. On the basis of our theory, this reaction to a sensation takes the form of a feeling. Between the sensation and the feeling must be interposed some circumstance (if the mind be autonomous) which will permit us to expect that a given sensation will not invariably give rise to the same feeling. In other words, the mental environment becomes a conditioning factor in the rise of the sensuous feelings. This environment we call the state of consciousness existing at the time. Hobbes was certainly correct when he said that to have always the same state of consciousness and to have none at all were one and the same thing. We assume, therefore, a constant ebb and flow in consciousness, which may dwindle in content to a mere point or, again, may diffuse itself like dim twilight. This movement is expressed by the term attention.

Recently it has been suggested that "the amœba's conscious experience may be rather a series of 'flashes' than a steady stream" on the supposition that "there are no trains of ideas to fill up possible intervals between the occurrences of outside stimulation."<sup>1</sup> Perhaps this question need not be raised, because, to the organism concerned, the intervals between flashes probably are non-existent. Moderate continuity in the phenomenal flow would suffice. If, however, the amœba's experiences really appear to it as a series of flashes, then its consciousness can hardly be more than a kind of mirror.

A mirror-type develops also if we proceed on the assumption that, in the amœba, attention and inattention are meaningless terms. "Different moments of its consciousness," writes Miss Washburn, "may differ in intensity; but attention, involving, as it does, clearness rather than intensity, arises only when mental states have become complex and possess detail and variety in their structure."<sup>2</sup> Of course the amœba may not have reached the stage of conscious autonomy, but investigations appear to testify rather to the opposite opinion by showing that probably the requisite detail and variety are not lacking. To quote Jennings: "Even the naked protoplasm of *Amœba* responds to all classes of stimuli to which any animal responds."<sup>3</sup> A most natural guess is that in the absence of attention, dependent on degrees of clearness, the bodily movements would be stereotyped reflexes and the mind would belong to one of the mirror-types. That this is not the case even with the amœba, experimenters are becoming more and more convinced. Complex situations in such a mind are likely rare, but with a moderate degree of content, the stream of consciousness should begin to resemble our field of vision, with a clear and distinct area (Blickpunkt), a broader, less distinct area (Blickfeld), and beyond this a 'fringe' in which the content shades off into total obscurity. Organic sensations should provide a fairly continuous background. To conclude: A small amount of complexity, differentiated in clearness as distinguished from sensory intensity, is

<sup>1</sup> Washburn, *The Animal Mind*, 1909, p. 49.

<sup>2</sup> *Idem*, p. 49.

<sup>3</sup> Jennings, *Behavior of the Lower Organisms*, 1906, p. 261.

(discontinuity)?

assumed as a necessary trait of any mind capable of manifesting its presence and thus existing as an object for science.

2. *Sensations.* — Irritability is a possession of all organisms, and actual contact is its primitive mode. No sense is telesthenic, not even sight or hearing. All stimuli act on organisms by contact. To none of the externally aroused sensations should we accord, in the beginning, any preëminence in virtue of its intensity or its quality; none provides any special endowment for overcoming the difficulties of the earliest stage of psychogenesis. In man, however, attendant circumstances differentiate certain internally aroused sensations from all other groups; these we call muscular sensations. No importance is attached to the fact that they are associated with contractions centrifugally produced, nor should conclusions be based on the doubtful presence of feelings of innervation when such contractions occur. But any organic movement is liable to cause sensations not only from the parts moved but also from the surfaces of the body, secondary results of the motion. This feature may early serve as a mark of distinction in consciousness between occasions when the organism is acting and when it is being acted upon. I shall employ the terms, tactual and muscular sensations, the latter to refer to sensations attending movement centrally initiated. All sensations may be regarded as formal and abstract, for whatever concreteness they actually possess is due to their affective accompaniments.

3. *Feelings.* — However much variety there may be among the sensations owing to their distinguishable qualities and intensities, the feelings are still far greater in number. Any feeling by its presence in consciousness is a guarantee that some sensation is present, but no specific feeling is guarantee for a particular sensation. Sensations may coexist, but no two feelings can coexist separately in the same consciousness, though one feeling may rapidly succeed another. Lists are extant purporting to comprise all the kinds of simple, irreducible feelings to be discovered in human consciousness, and from these may be selected such as seem indispensable for the lowest autonomous type of mind. According to the theory of Wundt there are three pairs of simple feelings: pleasantness and unpleasantness;

excitement and depression ; strain and relaxation. These feelings are not to be regarded like notes in a musical scale, either as sounded always separately or as always forming musical chords. Each pair really stands for a continuous series of feelings indeterminate in number and ranging between opposites. The linear series, taken together, stand as coördinates in a continuum of three dimensions. Controversy has arisen as to whether this classification may be reduced to a system of fewer dimensions. Royce, retaining the pleasurable and unpleasurable feelings, holds that the other four can be represented by a single other dimension ranging between restlessness and quiescence. We do not adopt this proposed simplification because the result would give no sufficient basis for an effective unit-concept.

We shall assume but four fundamental feelings : those of strain and relaxation ; of excitement and depression. Then, guided by facts of human experience, we define, as follows :

- (1) The feelings of *strain* accompany a large *content* of sensation in consciousness ; the feelings of relaxation are the reactions when the conditions causing the strain are removed.
- (2) The feelings of *excitement* accompany a large amount of *change* in the sensory content, requiring or producing a rapid shifting of the attention ; while the opposite feelings of depression are the reactions when the causes of the excitement are removed and consciousness becomes again more static.

It might be urged against the first definition that a feeling of strain often occurs although very little content be present, as when one is tensely awaiting the advent of a slight sound in stillness. Such strain, however, is largely sensory, due to muscular and organic alertness. Moreover, the experience implies a mind capable of selective attention to weak stimuli about to come. To a primitive organism there can scarcely be this interest in weak stimuli, and besides, it is a natural supposition that for such an organism the feelings in the beginning

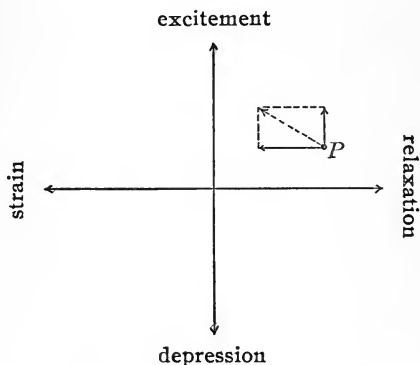
would be by no means prophetic or convey a reference to the future.

It may also be remarked that we omit the series of pleasurable and unpleasurable feelings. Psychologists often treat these feelings as preëstablished indices for the guidance of the mind in its choice of the foods which will prove beneficial to the body. There is something indeed miraculous in the function accorded to them, and the present writer disbelieves in it. Certainly the first feeling of pleasure must be entirely different from every previous experience, and we may despair of accounting for its being what it is; on the other hand, psychologists should not give up hope of discovering a reasonable explanation for its being attached to certain kinds of experiences while disagreeable feelings normally accompany certain other kinds. It is easy by complicating the discussion to avoid the main issue but the fact remains that, without appeal to the supernatural, the feeling of pleasure must be supposed at first to arise merely as an effect, and only later to assume the rôle of a cause influencing bodily behavior.

This, then, is our inventory of the indispensable factors in an autonomous mind: the four varieties of feelings mentioned above, and a mass of sensations, undifferentiated but soon to become so, the earliest line of cleavage among them being that which sets apart the muscular sensations which in man accompany what are called voluntary movements. In animals these sensations are attended by feelings of active strain, whatever those words may stand for. Let us next inquire how such a primitive mind would work, in order to gain some conception of its autonomy. To this end we shall follow the course of a typical process beginning with an external stimulus and ending in a bodily movement.

A stimulus, acting, does not insure the appearance of any sensation in consciousness. Rejecting the absurdity of unconscious sensations, we shall confine ourselves to those that attain the field of attention where they will gain various degrees of distinctness. Their dynamic value is probably measured by distinctness, which, however, is not always proportional to the intensity of the stimulation that causes them. Up to this point

the facts are conceivably explainable in physiological terms, so that the description might be taken as referring to a mechanical process.



If there is truth in this autonomous conception of mind it finds expression in the interaction of the incoming sensation with the inner state of feeling prevailing at that moment of time. In the above diagram we find a two-dimensional system representing all the primitive states of feeling arranged with respect to coördinates, so that the character of any prevailing affective state can be represented by the position of a point ( $P$ ). This point, in order to conform to the actual condition in consciousness, must be conceived as continually moving, since 'to have one state of feeling continuously is the same as having no consciousness at all.'

Treating now any single incoming sensation as an isolated fact, we may signify the change to the next prevailing state of feeling (due to the advent of the sensation) by the shift produced in the position of the point. If the sensation contributes somewhat to the amount of *change* going on in the sensory content, it will either augment or produce a feeling of excitement, and this may be represented by a displacement of the point ( $P$ ) upward; if it contributes to the *quantity* of the sensory content, this will make a displacement of the point in the direction of the feelings of strain, *i. e.*, toward the left of the diagram. The actual displacement might be represented by a resultant as determined by the familiar parallelogram of forces, *if we were dealing with a mechanical problem.* But this pro-

cedure would be in violation of the facts of consciousness as presented to immediate experience. Mental states obey a principle of relativity according to which any state comes to be what it is by virtue of all the past states with which it stands related. A state is profoundly conditioned especially by the states immediately preceding. To this may be added the law of contrast, applicable to special cases, according to which states closely related in time have their opposite qualities intensified by virtue of their proximity to one another. There are no laws such as these in physical nature, so the idea of a parallelogram of forces must be greatly modified before it suits the conditions of a mental environment.

Retaining the term 'parallelogram' as suggestive of its use, although no longer true of the shape of the figure, we may say, in reference to psychical conditions, that two parallelograms of different sizes but developed from the same position ( $P$ ) will not be geometrically similar figures but will generally suffer unlike distortions. Likewise, two parallelograms of approximately equal size but developed from different positions of the point ( $P$ ), will not likely be similar but will suffer unlike distortions. Added to this very complicated system of relations, there is probably, even when the sensory content remains relatively constant, a general drift or flow in the stream of affective states. Sufficient, no doubt, has been suggested to show that the point ( $P$ ) moves in a territory which as yet has been very slightly explored. The attempt to use a diagram also shows how hopeless it is to try to express in quantitative or spatial terms the qualitative phenomena of the mind.

Another feature suggested by the use of our diagram is, that feelings of excitement and strain, when intensified, approach a limit at which a relapse takes place, and either consciousness fades out or else feelings of an opposite character usurp the field. We need make no assumption about any regulative principle such as a will, whereby a mind may control its own states. If the external agencies causing the stress in consciousness continue and perhaps increase in activity, the finale is the destruction of the organism. For this reason, a propitious environment is required for the maintenance of a species.

On the other hand, there is implicit in the autonomous mind the possibility of 'continuous adjustment of internal relations to external relations.' But to deal with this topic, we must step outside of the 'charmed circle' of consciousness, in order to consider the feelings in relation to corresponding bodily movements.

2. The body of a living organism is always moving, and every movement may be regarded as the result of contraction. The so-called expansive movements are no exception, so that the correspondence between feelings and bodily behavior is reducible to the simple relation of feelings to contractions. These contractions produce effects in consciousness somewhat similar to the effects of external stimuli in that contractions generate sensations by the contact and pressure of parts of the body against one another and against external objects. Therefore, an intense state of feeling always indicates the presence either of stimuli or contractile movements. The situation requires, then, only the undisputed fact that stimuli give rise to bodily movements, in order to make clear the regulated mode of behavior which is observed in all lower organisms. The real connection is often masked by the fact that active contraction may cause inhibition of movement instead of movement. So far nothing is disclosed but what can perhaps be explained in physiological terms. What then is the function of autonomous consciousness in respect to behavior?

The reply seems to be that consciousness by virtue of the shifting of its states of feeling in accordance with its own peculiar laws introduces greater variety into the series of possible responses to stimuli than would arise through mechanical causes acting alone. For every sudden violent stimulus there is probably some immediate stereotyped response which may remove the cause and suspend action; if, on the other hand, the causes continue in the form of sensations, then consciousness, not the environment, determines the character of the further responses. The range of the possible responses is of course limited by the anatomical structure of the organism concerned, and in the case of the lowest organisms the number of these responses is indeed often very small, so that external evidence



for a mind in such cases is necessarily meager. But there is sound judgment in the remarks of Jennings, that "we usually attribute consciousness to the dog, because this is useful. . . . If *Amaba* were so large as to come within our every day ken, I believe it beyond question that we should find similar attribution to it of certain states of consciousness a practical assistance in foreseeing and controlling its behavior."<sup>1</sup>

The practical manifestation of such a mind as I have tried to outline is clearly expressed in the words of Dr. C. S. Minot: "The function of consciousness is to dislocate in time the reactions from sensations."<sup>2</sup> When mind intervenes between stimulation and the muscular reaction, the sequence is broken up, or as here expressed, is dislocated in time. One of a variety of results may follow stimulation: either inhibition may occur and no response appear; or the forthcoming response may occur but in a modified form; or else a movement may occur when no stimulus is actually present, presumably in response to a stimulus that acted sometime before. The reason for this variety of consequences, as I venture to believe, is found in the different affective attitudes which consciousness presents toward its experiences at different times.

The mind from these statements appears in the rôle of a revolutionary agent bent upon breaking up the ancient reflex order of things. But to assume off-hand that all consciously-directed movements are the successors of simpler reflexes may be a case of putting the cart before the horse. At least the order of succession may be reversed; reflex acts may be regarded as the consequents of conscious acts. Driesch writes: "When I first tried, six years ago, to classify organic movements according to their degrees of complication, it seemed inevitable that the classification should start from two types, which in different respects are the most simple ones: the so-called *simple reflex*, and the simple free directive motion called '*taxis*.' Modern investigations have proved that these two groups of movement, though the most simple in concept, are far from being the most fundamental in fact, and therefore a classification

<sup>1</sup> *Op. cit.*, p. 337.

<sup>2</sup> C. S. Minot, 'The Problem of Consciousness in its Biological Aspects,' *Science*, N. S., Vol. XVI., p. 1.

of organic movements at the present day will have to follow other lines of analysis.”<sup>1</sup> Jennings, after his thorough and patient study of animal behavior, says: “Each stimulus causes as a rule not merely a single definite action that may be called a reflex, but a series of ‘trial’ movements, of the most diverse character, and including at times practically all the movements of which the animal is capable.”<sup>2</sup> As long ago as 1889 Wundt expressed the conviction that practice consists in making original acts, which were conscious and voluntary, more mechanical and finally automatic.<sup>3</sup> In the *Grundzüge* we read: “The reflexes are voluntary acts grown mechanical.”<sup>4</sup> Titchener from these statements has developed the beautiful theory that mind is as ancient as life, that the first organic movement was a conscious movement.<sup>5</sup> If this were proved, it would mean a complete reversal of the old theory of behavior. All our unconscious actions—the beating of the heart, the passage of nourishment through the body, and all mechanical reflexes—would then be viewed as the direct descendants of a primal act consciously directed. Impossible of proof it is however, for we see but a part of the cosmic process and cannot generalize safely concerning the whole of it.

Nevertheless, whatever may be or may have been the condition at the lowest rungs of the ladder of organic evolution, the human mind attests the fact that at some stage below us consciousness became a factor in the process. A serviceable hypothesis relating to the mind at that stage of development is the goal toward which by our unit-concept we should endeavor to approach.

<sup>1</sup> *Op. cit.*, p. 8.

<sup>2</sup> *Op. cit.*, p. 280.

<sup>3</sup> Wundt, *System der Philosophie*, 1889, p. 548.

<sup>4</sup> Wundt, *Physiol. Psychol.*, 4te Aufl., 1893, Bd. II., p. 591.

<sup>5</sup> Titchener, ‘Were the Earliest Organic Movements Conscious or Unconscious?’ *Popular Science Monthly*, Vol. LX., No. 5, p. 458.

# SOME EXPERIMENTS WITH REACTIONS TO VISUAL AND AUDITORY STIMULI.<sup>1</sup>

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'Simple' reaction times with auditory stimuli are commonly found to be significantly shorter than reaction times with visual stimuli, under otherwise identical conditions, although there are exceptions to the rule.<sup>2</sup> The explanation of this delay of the visual reaction as compared with the auditory reaction is as yet a matter for speculation, and we do not know whether it is to be referred to the interval between peripheral stimulation and cortical process, to apperception differences, or to motor inhibition. This paper is a preliminary report of methods and results of experiments bearing on this problem.

The specific points we have so far attacked are:

1. Reactions with simultaneous sound and light stimuli, when the attention is exclusively directed to one stimulus (sound or light), as compared with the reactions to sound or light stimuli alone.

2. The discrimination of light from sound, when there is reaction to one stimulus and not to the other, and the discrimination of the combination of the two from either separately, when there is reaction to the combination and not to the single stimulus.

4. The discrimination of light or sound alone from combination of the two, when there is reaction to the single stimulus and not to the combination.

The apparatus employed can be described here only briefly. The Hipp chronoscope was used in the first two groups of experiments. The chronoscope was checked by means of the

<sup>1</sup>From the psychological laboratory of the John Hopkins University.

<sup>2</sup>See Wundt, *Physiol. Psychol.*, 5th ed., III., 414, 416. Angell and Moore, *PSYCHOL. REV.*, III., 245-258.

large Wundt fall-hammer in connection with a spark-chronograph, and ran with an average error of less than two sigma, which is small, when considered in light of the fact that an error of nearly one sigma in the reading was always possible. All our records may safely be said to be within two sigma of accuracy.

In the first day's work with the third group of experiments an accident disabled the chronoscope, and hence the records in this group were taken with the spark chronograph. We used the Schumann chronograph with the magnetic markers removed from the carriage, the 250-d.v. fork only being retained.

The visual stimulus was the illumination of a white screen by the flash of a helium tube. A Geissler tube could not be used, on account of the noise accompanying its flash. The helium tube flashes at lower pressure than a Geissler tube of the same length, and with no noise perceptible to the keenest ear, even when the ear is applied directly to the tube. The room in which the screen was placed was so darkened that the screen could barely be made out when the flash did not illuminate it. The reactor was given five or six minutes for adaptation. Under these conditions the flash illuminated the screen with a bright light which was not perceptibly tinged with color.<sup>1</sup> The screen, 16 × 20 inches square, was suspended vertically before the reactor, with center about on a level with his eyes, and approximately a yard therefrom. The tube was on a level with the top of the screen, and fifteen inches in front of it, shielded from the eyes of the reactor. The illumination of the screen appeared uniform. Early in the experiments an attempt was made to use the flash diffused through ground glass, but this was not found satisfactory.

The auditory stimulus was a snap in a telephone receiver which in the first two groups of experiments was held over the reactor's ear by an ordinary operator's headpiece, and in the third group was supported on a rod close to the reactor's ear.

A large Ruhmkorff coil was used to produce the spark for the operation of the helium tube. A specially constructed key, by one pressure of the operator's finger first closed the primary

<sup>1</sup> The illumination was really yellowish.

circuit through the spark coil, and a moment afterwards broke that circuit and simultaneously completed the circuit through the chronoscope magnets. The current was supplied by storage cells, and kept constant by the aid of volt and ammeter readings.

An automatic cut-out, designed and constructed for the purpose, 'shorted' the secondary circuit of the coil at the closure of the primary circuit, so that the 'make' spark did not affect either the helium tube or the telephone circuit described below. Thus the 'break' spark which occurred at the moment of the closure of the chronoscope circuit was alone used. Physical measurements showed that the delay of the spark after the actual break of the primary circuit was less than one fourth sigma.

A spark gap of adjustable width was inserted in the spark circuit, and shunted around the gap was the nominal secondary circuit of another and smaller inductorium. The nominal primary coil of this inductorium was connected with the telephone receiver, and hence by the passage of the spark from the large inductorium a current was generated in the telephone circuit, causing a sharp snap of the diaphragm. By varying the width of the spark gap, and so varying the amount of spark current sent through the smaller inductorium, the loudness of the auditory stimulus could be finely controlled. In practice the sound was made decidedly loud: but not disagreeably so.

The telephone receiver could be completely disconnected from the coil by the opening of a double-pole switch. By means of another switch the whole spark current could be turned off the helium tube and turned on a Geissler tube in the operator's room. Thus, at the option of the operator, auditory stimulus alone, visual stimulus alone, or both together could be given to the reactor.

The automatic key described in *Psychological Monographs*, Vol. 10, No. I., 26-37, was used throughout. A catch, holding the key down when completely depressed, was added. This catch did not interfere with the automatic action of the key, and by keeping the circuit broken until released by the reactor, gave the operator time to release his key, without the necessity of instructing the reactor to hold the key down for a moment after

reacting. The automatic key enabled the subject to make his attention essentially 'sensory,' as will be described below.

When the spark chronograph was substituted for the chronoscope, the primary circuit through large inductorium and reactor's key was broken mechanically by the revolution of the drum, and immediately (in ten or twelve sigma, that is) remade. The same spark which traversed the helium tube (or Geissler tube, in case there was no visual stimulus) was carried from tuning-fork stylus to drum. The reactor, by again breaking the primary circuit, produced a second spark, and of course a second stimulus. This was a condition not present in the first arrangement, with the chronoscope.

In work following that reported here a different arrangement will be employed. The auditory stimulus will be the noise of the spark leaping a gap, and there will be a separate inductorium in the reaction circuit, so that the reaction will not be accompanied by a repetition of the stimulus.

The subject was given a tactual warning or preparatory signal. A specially constructed instrument held in the left hand vibrated when an interrupted current was sent through it. The vibration was commenced approximately two seconds before the reaction-stimulus, and continued approximately half a second. The subject could communicate with the operator by tapping on a Morse key placed beside the reaction key.

The room in which the subject was placed was separated from the operator's room by a hall way. Both doors being closed, the operation of the stimulating and recording apparatus could not be heard by the subject.

The reactor's attention was 'sensory' throughout. After the first day or two there was no consciousness of the hand or expectation of the movement after the warning signal, and very little consciousness of the reaction or the hand between reactions, except in the case of the discriminative reactions. The attention, after the warning, was exclusively to the prescribed stimulus, and the effort was simply to perceive it at the earliest possible moment. Such at least was the definite introspection of three of the four reactors. The reactors were warned not to hurry the hand or otherwise to interfere with it, and instructed

to let it and the reaction alone, and to attend strictly to the stimulus after the warning signal. Complete 'sensory' attention at an early stage of an experiment is possible only with an automatic key. With an ordinary key the reactor must give a certain amount of attention to the hand until he has had long practice.

There were four reactors in these experiments: two graduate students, G. R. Wells and H. M. Johnson; an undergraduate, J. Marston; and the author.

Averages mean very little in reaction time results. Mean variations mean still less. The method of Tigerstedt, which groups the reactions according to the limits within which they fall is especially good, but convenient comparison and summarization of different experiments is difficult if dependent on the primary groupings. Plotting the graphs based on the groupings (Alechsieff's method) helps the eye, but does not compensate for the space and labor it requires. Graphs also lend a fictitious importance to the results, as there is no essential relation between the ordinate and abscissa, hence the experimenter usually selects a scale of the two which gives an imposing height to the graph as compared with its width. A similar remark about the use of percentages instead of actual number of experiments in the groupings of Tigerstedt's method, is quite legitimate.

We give in the statements below, the averages for the daily groups of experiments, with distribution tables for the total number of reactions of each class for each subject. In addition, we give the limits within which certain percentages of the total number of reactions fall. The percentages selected, as a result of careful study of these and other results, are 80 and 50. While this selection is arbitrary, we believe it will be found that the percentages indicated, when determined by the rules set forth below, are the most significant in the greater number of cases. The rules for determining the limits are as follows:

1. Take the fewest consecutive groups which together include 50 per cent. or more of the total number of reactions.
2. Take the fewest consecutive groups which together include 80 per cent. or more of the total number of reactions.

3. If in any case there are two sets, of equal number of groups, which include 50 per cent. or more (or 80 per cent. or more) of the total number, take the group containing the higher percentage. If the two groups contain equal percentages, take the group containing these two.

The application of the last portion of the third rule is illustrated in the case of the *F-dsf* results of Reactor W. 210-270 and 230-290 each include 51.1 per cent. Hence the group taken must be 210-290, including 61 per cent.

#### 1. REACTIONS WITH SINGLE AND COMBINED STIMULI.

Four classes of reactions were produced in these experiments, according to the following conditions of attention and stimulation. (*S*); sound stimulation only. (*F*); visual stimulation only. (*Sf*); combined stimuli, but reaction to the sound — the attention in the preparatory interval being directed exclusively to the sound. (*Fs*); combined stimuli, but reaction and attention to the flash.

Ten reactions of each class were taken in succession, two to four reactions being allowed before beginning the series for the reactor's adjustment, and not recorded. One series from each class, forty reactions in all, constituted a period's work, requiring from thirty to forty minutes. Usually only one period's work was done in a day by one subject; in a few cases two periods of work were done, but not in immediate succession.

The four classes of series were taken up in eight standard orders, to equalize the effects of position in the set. These orders were: I., *F-Fs-S-Sf*; II., *S-Sf-F-Fs*; III., *Sf-S-Fs-F*; IV., *Fs-F-Sf-S*; V., *Sf-F-Fs-S*; VI., *Fs-S-Sf-F*; VII., *S-Fs-F-Sf*; VIII., *F-Sf-S-Fs*.

Through a mistake series in three other orders were included. These sequences were: A, *Sf-S-F-Fs*; B, *Sf-F-S-Fs*, and C, *F-S-Sf-Fs*.

The reactors reported that in the cases of *Sf* and *Fs* attention was almost without exception to the indicated stimulus, and the other was in consciousness only after its presentation. All reported that the reactions to *F* seemed much slower than the reactions to *S*. The reaction to *S* seemed to follow directly



upon the stimulus, as viewed in retrospect, while there seemed to be a slight pause or hesitation in the case of the reaction to *F*. Several persons, not concerned in the experiments here reported, have made similar observations in this laboratory. This introspection is of no little importance.

The averages of the daily series in this experiment are given in Table I.

TABLE I.  
AVERAGES BY SERIES, IN  $\sigma$ .

Reactor.	Ser.	Seq.	(S)	(F)	(S')	(F')
J.	1	II.	174.9	176.1	146.2	152.8
	2	II.	106.7	170.3	101.0	116.2
	3	II.	105.0	146.5	81.09	113.0
	4	III.	93.7	133.2	89.2	103.9
	5	A	94.6	148.9	101.9	102.6
	6	B	73.4	139.0	89.3	93.9
	7	III.	84.4	161.7	77.5	72.0
	8	VII.	82.2	179.3	101.5	113.9
	9	I.	88.2	138.9	104.9	96.7
	10	V.	119.2	150.8	123.3	136.8
	11	VIII.	104.0	165.3	109.9	125.1
	12	VI.	99.7	159.6	108.6	109.9
	13	IV.	166.4	155.0	157.0	143.3
M.	1	I.	116.9	165.8	117.2	147.5
	2	C	92.0	150.2	95.7	132.7
	3	VIII.	104.8	159.3	97.0	126.7
	4	I.	82.5	153.3	84.1	116.7
	5	VI.	117.4	154.9	88.3	134.5
	6	III.	100.7	151.6	109.3	127.7
	7	II.	97.4	162.8	87.0	107.2
	8	IV.	74.7	157.2	62.7	105.5
	9	V.	100.1	166.4	83.2	94.5
	10	VII.	81.5	164.8	118.3	110.6
W.	1	IV.	127.2	176.7	149.3	197.0
	2	III.	159.2	178.7	122.7	144.2
	3	II.	120.4	178.9	111.8	149.6
	4	V.	131.4	164.0	123.9	141.9
	5	I.	135.5	178.0	123.1	138.3
	6	V.	82.1	166.2	105.1	124.8
	7	IV.	118.9	162.3	110.9	138.5
	8	VI.	98.4	186.2	130.0	135.3
	9	VII.	103.0	186.6	132.9	128.4
D.	1	II.	96.2	156.5	99.7	109.2
	2	III.	124.0	195.9	127.9	116.8
	3	VI.	116.5	199.1	140.0	149.7
	4	D	79.8	187.7	92.8	114.1
	5	I.	108.6	161.0	117.9	107.9
	6	IV.	104.6	140.5	106.4	107.5
	7	V.	92.6	134.7	110.1	93.0
	8	V.	116.9	150.7	116.7	155.3
	9	VII.	135.5	166.1	115.1	130.8
	10	VIII.	96.1	174.1	95.3	110.8

TABLE II.

DISTRIBUTION BY GROUPS OF 100 DIFFERENCES.

Times.	Reactor J.				Reactor M.			
	S.	F.	Sf.	Fs.	S.	F.	Sf.	Fs.
From	1	0	0	1	0	0	3	0
40	2	0	3	3	3	0	4	0
50	2	1	3	1	4	0	0	4
60	5	0	4	4	5	0	6	2
70	14	1	11	7	12	1	11	2
80	19	0	12	10	10	0	13	4
90	19	1	18	15	21	0	15	13
100	19	1	27	18	17	1	16	12
110	14	1	20	16	9	1	9	21
120	9	5	4	18	9	2	8	8
130	2	13	10	14	7	3	6	17
140	8	21	6	7	2	18	2	12
150	4	32	3	3	0	28	2	4
160	3	24	3	5	1	22	0	3
170	4	15	1	4	0	15	0	1
180	1	7	0	1	0	6	0	1
190	0	1	2	0	0	2	0	0
200	1	0	1	1	0	1	1	0
210 up	2	2	0	0	0	0	0	0
Totals	129	125	128	128	100	100	100	100

Reactor W.				Reactor D.				
From	1	0	2	0	1	0	1	0
50	1	0	2	1	0	1	1	1
60	5	0	2	1	1	0	1	2
70	5	0	2	1	7	2	6	5
80	6	2	5	3	13	0	2	6
90	9	1	4	3	13	0	10	11
100	15	0	7	10	24	0	32	18
110	4	0	10	6	17	0	17	19
120	13	1	12	11	12	1	16	10
130	12	11	16	14	5	6	5	9
140	4	2	10	14	2	16	3	7
150	2	12	9	5	2	23	2	4
160	6	11	3	4	1	11	2	3
170	0	17	2	4	0	7	0	2
180	3	10	1	5	0	12	2	0
190	1	5	2	3	1	6	0	3
200	2	5	1	2	0	6	0	0
210	0	3	0	2	0	4	0	0
220	0	3	0	0	0	1	0	0
230 up	1	7	0	2	1	5	0	0
Totals	90	90	90	90	100	101	100	100

Table II. gives the distribution of the individual reaction times in groups differing serially by ten sigma. The numbers in the first vertical column indicate limiting lengths of reaction times. A number in one of the following columns indicates the

number of reactions, of the class indicated by the column heading, which fall within the limits of the time opposite which the number is placed and the next higher designated time, reactions falling on the even time being counted in the group of which that time is the lower limit. Thus, the number 14 in the fifth horizontal line below the headings, in the second vertical column, indicates that of 129 reactions to sound alone by reactor J., 14 reactions were 70 sigma or more in length, but less than 80 sigma.

The limits within which fall percentages of reactions nearest above 50 per cent. and 80 per cent. of the totals given in Table II. are given in Table III.

TABLE III.  
DISTRIBUTION OF CRITICAL PERCENTAGES.

Reactor.	<i>S</i> .	<i>F</i> .	<i>Sf</i> .	<i>Fs</i> .
J. Total	129	125	128	128
Per cent.; $\sigma$	86; 60-160 55; 80-120	84; 130-180 61; 140-170	80; 70-140 50; 90-120	82; 70-150 50; 90-130
M. Total	100	100	100	100
Per cent.; $\sigma$	85; 60-140 57; 80-120	83; 140-150 50; 150-170	84; 60-140 53; 80-120	83; 90-150 58; 100-140
W. Total	90	90	90	90
Per cent.; $\sigma$	84; 70-170 58; 90-140	80; 130-210 54; 150-180	80; 80-160 53; 110-150	80; 100-190 50; 110-150
D. Total	100	101	100	100
Per cent.; $\sigma$	91; 70-140 54; 90-120	86; 130-210 55; 140-170	80; 90-140 65; 100-130	85; 70-150 58; 90-130

The differences between the auditory and visual times are considerable. The fact that reactions to *Fs* are very little longer than reactions to *S*, although the attention and predetermination of the reaction in the former case were to the flash alone, may possibly be taken to mean that the reaction was really to the sound. This explanation is not indicated by the experiments described in the next section. The experiments with the *Sf* stimulus were valuable chiefly because they enabled the reactor to control his attention in the experiments with *Fs*.

## 2. REACTIONS WITH DISCRIMINATION.

For the purpose of throwing light on the relation of the *Fs* reaction to the *F* reaction and the *S* reaction, experiments were

made on three subjects by giving in irregular sequence *F*s and *S* stimuli, and requiring the subject to expect the *F*s stimulus, premeditating reaction to that and not to the *S* stimulus. Each series was composed of twenty of each type of stimuli, given in the order determined by the shuffling of a pack of cards. Reactor D. was given this type of discrimination test alone. Reactor J. was given in the same period a series of this kind with another series in which *F* and *S* stimuli occurred, reactions being to the flash. These two types of series, which we will designate as *Fs-ds* and *F-ds* respectively, were given in reversed order on alternate days, that is, on one day the *F-ds* series was given first, and on the next day the *Fs-ds* series first. Series of *S* and *F* stimuli, in which reactions were to sound and not to flash, were given to reactor *W*, but not on the same days with the *Fs-ds* and *F-ds* series. This third type of series will be designated *S-df*.

Contrary to our preconceived notions, the attention in these experiments remained 'sensory.' The understanding and resolution before beginning a series that the reaction was to be to one stimulus and not to the other, with at first a reversion to this consideration in the intervals between experiments, was all the needed consciousness of the motor process itself. This introspection became unquestionable in the discrimination experiments which are described in the next section.

The averages of the reactions in the *S-df*, *Fs-ds* and *F-ds* series are given in Table IV., and the distribution of the critical percentages in Table V.

Two things are shown by these meager results. First: that the shortening of the visual reaction time when an auditory stimulus accompanies the visual stimulus is not explicable as due to reaction to the sound instead of the flash, for the reaction to *F*s is generally shorter than the reaction to *F* when both are discriminated from *S*. Second (in the case of reactor *W*.): the time of reaction to *S* discriminated from *F* is shorter than the time for reaction to *F* discriminated from *S*. The reaction time for *S* discriminated from *F* is actually shorter than the 'simple' reaction time for *F*. This same relation was found to exist in results of short series obtained from the other reactors,

TABLE IV.

AVERAGES BY SERIES.

Reactor.	Ser.	<i>F<sub>s</sub>-ds.</i>	Errors.	<i>F<sub>s</sub>-ds.</i>	Errors.	<i>S-df.</i>	Errors.
W.	1	213.3	0	209.2	0	157.4	0
	2	185.5	0	202.1	0	125.5	0
	3	179.5	0	198.8	2	128.0	1
	4	—		—		177.0	0
J.	1	209.1	1	186.0	0		
	2	156.3	1	173.8	0		
	3	153.9	4	175.2	4		
	4	140.8	1	157.6	1		
	5	139.8	2	167.7	0		
	6	130.1	2	158.6	0		
M.	1	—		167.0	3		
	2	—		181.5	0		
	3	—		167.3	0		
	4	—		147.5	2		
	5	—		160.6	1		
	6	—		157.8	2		
D.	1	169.0	0				
	2	132.7	8				
	3	137.1	2				
	4	139.2	2				

TABLE V.

DISTRIBUTION OF CRITICAL PERCENTAGES.

Reactor.	<i>F<sub>s</sub>-ds.</i>	<i>F-dS.</i>	<i>S-dF.</i>
W. Total	60	60	80
Per cent.; $\sigma$	80; 140-220 51; 160-210	80; 170-250 60; 170-220	83; 90-180 50; 120-160
J. Total	120	120	
Per cent.; $\sigma$	86; 110-200 56; 120-160	81; 130-190 59; 150-180	
M. Total		120	
Per cent.; $\sigma$		81; 130-180 50; 150-170	
D. Total	76		
Per cent.; $\sigma$	80; 100-170 55; 110-150		

but the data in these cases were not sufficiently numerous to warrant insertion in the table.

It is obvious that there is here a field for work from which the results will be exceedingly significant. Before undertaking comprehensive investigations of the points upon which we have

just touched we turned to a more difficult form of discrimination, and obtained some results which are detailed in the next section.

### 3. REACTIONS WITH INHIBITORY STIMULI.

The discrimination of light from sound, or *vice versa*, is not especially difficult. There is some reason for supposing that after a reasonable amount of practice the reaction to *F-ds* and *S-df*, without errors, might be practically the same as the 'simple' visual and auditory reactions respectively. The discrimination of *Fs* from *S* is decidedly more difficult: the discrimination of *Sf* from *F* we have not yet tried.

When we come to the discrimination of *S* from *Sf* and the discrimination of *F* from *Fs*, the difficulty increases enormously. In these cases the reaction-stimulus is given each time, but in half the cases is accompanied by the stimulus of the other mode, which is expected to inhibit the reaction.

Series were taken on all four subjects with the stimulations *F-dsf* and *S-dfs*. Each series consisted of twenty reaction-stimuli (*S* or *F* as the case might be), and twenty inhibition stimuli (sound and flash combined). These stimuli were given irregularly as determined by the shuffled cards. The spark chronograph was used in these experiments, making the operation more difficult than in the preceding series; and occasionally records were spoiled, so that the full number of twenty was not obtained in every series. Sometimes the operator became confused, and used the same card twice, so that in some series there were more than twenty reactions. Errors, that is, reactions to the combined stimuli, were noted by observing the spark, so that an injury to the records did not prevent a complete report of the errors in any series.

One series of each type, the order reversed on alternate days, constituted a day's work. The subject's attention was in all cases sensory. No effort was made to react in one case or inhibit the reaction in the other: the whole effort was put into the discrimination of the single stimulus from the combination of the two, and in the cases of three reactors (J., W., and D.) the way in which this was done was introspectively clear. The reactor expected the single stimulus (sound or flash), represent-

ing that stimulus as distinct from the combination, and attending positively to that representation. No attention was given positively to the combined stimuli although it may be inferred that in a negative way the inhibitory stimulus complicated the expectation. When this state of attention was maintained up to the moment of the stimulus, the reaction occurred if the stimulus was the expected one. If the stimulus was not the expected one, the reaction never occurred. But the requisite attention was not always maintained, being occasionally disturbed by noises from the rooms adjoining the subject's room, and if the attention to the reaction-stimulus was lost just before the stimulus, the reaction occurred without regard to which stimulus was given.

It would be desirable to obtain series in which there were no errors, but we succeeded in obtaining only a few of these. The reactors labored under especial disadvantage because of having been habituated to reaction to the combined stimuli in the earlier experiments. More work will be done with fresh reactors, and with the present ones after several months' rest.

The occurrence of errors does not necessarily discredit the series in which they occur. The pairs of series in which the errors are equal, and the pairs from which errors are absent, show in general the same relationships as the pairs in which there are in one series more errors than in the other. The effect which the disposition to error may have on the reactions to the right stimuli is in itself a problem which is not simple.

It is noteworthy that more errors occurred in the *S-dsf* series than in the *F-dfs* series. Reactors W. and D. found introspectively that the maintaining of attention was more difficult in the former series than in the latter. Reactor J. on the other hand reported that attention to *S-dsf* was the easier, although he thought that his attention to *F-dfs* was the better. This latter condition he said was due to the fact that he made more effort in the *F-dfs* series, which probably accounts for the divergence of his results from those of the other three reactors.

In the consciousness of all three subjects the sound became associated with darkness, and the light with silence, as positive content, thus becoming a complex expectation-content.

The introspection of Reactor M. did not differ in substance

from that of the others, but no stress is laid upon his reports here, because he was absolutely untrained in psychology.

The averages of the series are given in Table VI., and the distribution of the critical percentages in Table VII.

TABLE VI.  
AVERAGES BY SERIES.

Reactor.	Ser.	<i>F-dfs.</i>	Errors.	<i>S-dsf.</i>	Errors.
W.	1	247.1	0	293.7	0
	2	302.6	0	255.4	1
	3	282.0	1	240.7	1
	4	273.2	1	237.8	3
	5	246.7	2	238.2	1
	6	268.8	0	256.8	0
	7	258.2	0	259.4	0
M.	1	187.4	8	173.6	7
	2	167.8	5	133.8	8
	3	192.6	7	133.1	9
	4	168.5	8	137.9	12
	5	203.0	5	162.4	8
	6	222.4	2	224.5	2
	7	250.0	0	208.6	2
J.	1	187.0	4	184.2	9
	2	218.5	4	216.4	7
	3	208.3	3	219.9	8
	4	210.8	1	221.2	5
D.	1	244.1	2	208.5	4
	2	243.9	2	222.1	3
	3	256.7	5	220.1	4
	4	264.8	2	246.6	2
	5	292.3	0	220.4	0
	6	263.3	3	220.0	3

TABLE VII.

DISTRIBUTION OF CRITICAL PERCENTAGES.

Reactor.		<i>F-dfs.</i>	<i>S-dsf.</i>
W.	Totals	133	135
	Per cent.; $\sigma$	82; 200-320	82; 190-290
		61; 210-290	55; 200-270
M.	Totals	140	122
	Per cent.; $\sigma$	80; 120-250	86; 100-240
		51; 150-210	50; 120-170
J.	Totals	77	71
	Per cent.; $\sigma$	80; 170-260	83; 160-290
		54; 170-210	53; 160-220
D.	Totals	110	91
	Per cent.; $\sigma$	86; 200-310	94; 180-290
		54; 210-270	51; 190-220



Three reactors, W., M., and D., show a general difference between the two types of reactions; *S-dsf* giving a quicker reaction than *F-dfs*. The results of reactor J. show no material difference between the two types.

No definite deductions should be made from these results, although they open up interesting hypotheses. We hope to extend the experiments along the lines herein mapped out, and to carry out two other experiments on similar lines. The first of these will be an investigation of visual and auditory reactions with distractions, so arranged that there is no attention to either stimulus or movement until after the reaction. The results of this experiment will help to settle the questions suggested by results such as we have summarized above.

The second experiment will be on reactions to a visual stimulus whose duration (physiological and psychological) is more nearly of the same order as that of the auditory stimulus we have employed; reactions to longer auditory stimuli; and reactions to visual and auditory stimuli lasting until after the reactions are over. For these experiments a new set of apparatus is necessary. A third relevant experiment, on rhythmic reactions, is already in progress.

#### 4. CHECK EXPERIMENTS.

It is sometimes claimed that the Hipp chronoscope must be carefully leveled, or accuracy cannot be expected from it. We are not aware of any published experimental evidence on this point, hence we made some tests for our satisfaction. With a given adjustment of fall-hammer, current, and springs, readings were taken with the chronoscope approximately level. Then a strip of wood 1.5 cm. thick was placed under one edge of the base of the chronoscope, tipping it and readings taken in these positions. Table VIII. gives the results of this experiment.

TABLE VIII.

Chronoscope.	av.	m.v.
Approximately level.....	146.3	1.3
Left side raised.....	145.7	1.5
Right side raised.....	144.8	1.08
Front raised.....	146.7	1.16
Back raised.....	145.1	1.48

The figures given are averages of ten single tests. The experiment was repeated on another day with a different spring tension, with results of the same order. The tilting back or front is equivalent to a slight change in the tension of the springs, as may be readily understood from the construction of the chronoscope. What produced the slight shortening of the reading when the chronoscope was tilted to one side was doubtless the rubbing of the weight-cord against the side of the slot through which it runs. Tipping the chronoscope backwards or forwards did not bring the cord against the wood, on account of the direction of the slot. Side tilting of less extent does not produce any noticeable effect on the running time. It is clear that ordinary methods of leveling the chronoscope by eye are quite adequate.

To check the possible effects of variations in the intensities of the stimuli we employed, we made experiments with the visual stimulus, using three current strengths on the induction coil primary. The currents were: 3 amperes (the current used in the foregoing experiments), 1.75 amperes, and 4.5 amperes. The stronger current gave a flash distinctly brighter than that produced by the break of the normal current, while the weaker current was the lowest which would produce a dependable flash, and its flash was so pale that the reactors complained of it. Increases in current strength above 4.5 amperes produced little appreciable increase in brightness.

Experiments were made in series of ten reactions at each intensity, two series at each of the three intensities being taken in one period, in the following orders:

normal — strong — weak — normal — weak — strong  
weak — normal — strong — weak — strong — normal  
strong — weak — normal — strong — normal — weak

The averages for the twenty reactions of each sort of stimulus in each series are given in Table IX. The differences between reactions to the normal and to the weak intensities are considerable, but the difference between the two intensities of stimuli was large. Moreover, the reactor was conscious of the weak flash as being unsatisfactory by reason of its weakness. The differences between the reactions to normal stimuli and

those to stronger stimuli are inconsiderable. It is practically certain that no incidental variations in the normal stimuli employed in our work were of consequence.

TABLE IX.  
AVERAGES BY SERIES.

Reaction.	Series.	Weak.	Normal.	Strong.
M.	1	160.7	141.1	141.1
	2	157.5	148.0	144.5
	3	149.0	150.6	153.0
J.	1	184.2	179.6	171.9
	2	158.9	139.5	137.3
	3	159.8	147.3	144.5

The influence of stimulus-intensity on reaction times needs a careful reinvestigation. The results of the experiments which have been carried on in the past convince one of very little. That differences of intensity, as such, within limits of what may be called satisfactory intensities, may cause differences of reaction time is not established. By satisfactory intensities, we mean those to which the subject reacts without noticing that they are excessively weak, excessively strong, or inadequate in some other respect. Liminal intensities, or near-liminal intensities, and excessively strong intensities doubtless are not the equivalent of moderate intensities of reaction-stimuli, even aside from their specific effects on attention. In the cases of moderate intensities contrast, novelty, specific expectations, and similar factors modifying the attention attitude are possibly the only means through which differences in intensity may produce differences in the reaction times.

# THE COMIC AS ILLUSTRATING THE SUMMATION-IRRADIATION THEORY OF PLEASURE-PAIN.

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The comic and the tragic are in many respects the clearest exemplifications of the general law of emotion—the comic throwing especial light from the side of the conditions of pleasurable repose, the tragic from the side of the heightening of tone. The explosive character of these experiences, contrasted with the contemplative aspect which other forms of æsthetic enjoyment present, renders analysis comparatively easy. The culmination of the æsthetic moment is less intellectual, more sensuous, which means that it takes place in terms of grosser motor coördinations, whereas the æsthetic repose of beauty in other modes is a resolution in terms of accessory muscles, finer habits and adjustments, which do not arouse the elemental emotions or, when they do, subject them to a greater degree of control.

Up to a certain point the conditions of the comic and the tragic consciousness, as of laughter and weeping, are identical, as Darwin has sufficiently shown; but beyond that point they diverge, giving rise to distinct problems of dramatic art. It is not the purpose here to trace this divergence, but the connection is pointed out since, as will appear later, it is one of the corollaries of the present argument, that the make-believe of the tragic drama is essentially an expression of the spirit of Comedy.

## I. THE SUMMATION-IRRADIATION THEORY.

The pleasurable emotion which accompanies laughter exhibits the two fundamental phenomena of emotion as it exists everywhere: the initial summation of stimuli, followed by an irradiation or discharge which may take place either abruptly or in a more gradual way. The summation of stimuli is not peculiar

iar to pleasure, but is the condition of all emotional experience, being characteristic, as well, of pain. Thus the cumulative process in the case of sneezing or tickling readily passes from a pleasurable into a painful experience. On the other hand, the phenomena of irradiation seem to be more distinctive of pleasure, pain being due to the failure of summated stimuli to find such discharge.

Agreeable emotion is connected with such massing of stimuli as leads to a response within the normal limits of the functional capacity of the organism, while pain accompanies the piling up of stimuli and the subsequent discharge when these exceed the limits of such normal functioning. Thus moderate stimulation and exploiting of a habit are pleasurable, while any serious thwarting is painful: the habit in the latter instance may function, but this takes place under normal stress and beyond the limits of its ordinarily easy and therefore agreeable operation. Excitement and the overcoming of obstacles is pleasurable only if it results in the final triumph of a habit. Educational and other intellectual processes when kept within the confines of what is called interest are pleasurable, but carried beyond that point for the sake, it may be, of what is called mental discipline, these processes become irksome. In Mark Twain's entertaining story of the first pair, Adam and Eve in naming the animals are represented as deciding to call the toad a toad because it looked so very much like a toad. Why is it that this inane statement calls forth a smile when a scientific statement of the morphological characters of the arciferous tailless amphibian would elicit only a yawn? Because the former involves the falling back physiologically upon preëxistent habit-systems, functioning agreeably, while the latter involves their tensional reconstruction beyond the limits of such functioning.

Generalized in the more technical phraseology, the law of emotional experience has been expressed thus:<sup>1</sup> The conditions of pleasurable feeling are the irradiation, along lines of habitual response, of stimuli whose summation and discharge fall within the limits of the normal functioning of the organ or organs

<sup>1</sup> Bawden, 'The Nature of Æsthetic Emotion,' *PSY. REV.*, Vol. XV., pp. 265-291. Also in *Principles of Pragmatism*, Chapter IV.

involved. The physiological mechanism of laughter is one of these lines of habitual response, a prominent and important one in the case of the experience of the comic. But, in addition to the cruder mechanism of cacchination, there is the less well understood but equally important system of sensorimotor connections in the cortex, with their ramifications to the finer musculatures of optical, laryngeal, and facial expression. These are of permanent importance in the case of the comic as an æsthetic appreciation, since this experience seems to attain the highest level of development under circumstances in which the cacchinatory discharge is partially or wholly inhibited.

On the physiological side our knowledge of the sensorimotor adjustments here involved is still imperfect and incomplete, although the neurologists and experimental psychologists are laying the foundations upon which an explanation will some day be built. But the analytic psychologists have carried the study of the content of the consciousness of the comic to a point which, in certain directions, enables us to make significant correlations with such physiological knowledge as we do possess. A German psychologist, Zeising, has happily epitomized the successive stages in terms which enable us to interpret this analytic content in connection with the psychophysical processes: he describes the experience of the comic as 'tension, discharge, and recovery of poise as we free ourselves.' The tension or summation is expressed by the 'expectant attention,' 'great expectations unfulfilled,' of the classic treatises. The irradiation or discharge is seen in the typically abrupt or explosive character of the comic. The recovery of poise or control is the counterpart, in the comic, of what is elsewhere known as the æsthetic repose.

## II. 'TENSION.'

The phenomena of summation or tension are readily recognized on the physical side in the overt reactions or incipient innervations of the motor apparatus involved, such, for example, as the convulsive paroxysms of the diaphragm and the half-born grimaces of the facial muscles. Expectancy, a prominent element in every experience of the comic, illustrates at once the 'set' of the habit-systems and the summative stimulation by

which the abrupt kaleidoscopic reorganization is to take place. The half-suppressed laughter of the titter, the giggle, and the snicker illustrate the instability of the motor mechanism. The ticklish places, as Darwin points out, are those most exposed to attack, in the sense of being the most organically vital. The musculatures clustered about such places are, accordingly, in most unstable equilibrium. The intensity of the comic experience, as we know, is dependent in part upon the state of the habit-systems immediately preceding the climax of the stimulation. This summation of stimuli is the state of expectancy. Such a state may either develop an increasingly unstable equilibrium up to the moment of discharge, as in the case of the less intellectual forms of the comic such as humor, or a distinctly antagonistic attitude may be intermittently evoked, thus enriching the cognitive content of the final discharge, as in the case of the more intellectual forms such as wit.

Expectancy is invariably utilized by a good story-teller. It matters little how the attitude is aroused. Anything will serve which will throw the listener into a state of motor instability or, what is the same thing, into a state of incipient response. Many persons secure this by themselves laughing as they recount the anecdote or tell the funny story. The contagious gestures and facial expressions of the speaker go far to put the hearer into an attitude to respond readily when the climax is reached, and all know that when such a dynamogenic attitude has once been induced, one will laugh at events and remarks which under ordinary circumstances utterly fail to evoke merriment. The reason why the ludicrous does not bear too frequent repetition and sometimes none at all, is found in this principle of expectancy.

On the mental side, the tension appears in the contrasts, contrarieties, and incongruities of the ideational content, which may take the form of practical disadaptations, intellectual contradictions, or ethical conflicts. Practical inadequacies abound in the conditions of the comic, as illustrated by the ludicrous in situation and incident and by the practical joke. The difference between this class and logical incompatibilities is simply that in the latter instance the conflict takes a less concrete and overt form.

The determination of our understanding 'to form simultaneously two contradictory statements,' to which Dumont traces the comic, is simply a psychological statement of the conflict of habit-systems. The same may be said of the theory of Sully who says that "the jest must contain something that is capable of deceiving for a moment," and of Schopenhauer for whom "the phenomenon of laughter indicates the sudden perception of an incongruity between a conceptual and a real object." The humorous character of many lapses of speech turn on such an equivocation or contradiction in meaning. The words of the preacher who in the midst of his sermon exclaimed: "Have we not all of us at times felt a half-warmed fish within us!" would not have been comic if the substitution of initial consonants had not made a certain semblance of sense. It was the arousal of this incongruous conflicting association-complex connected with 'half-warmed fish' that led to the humor of the situation when its accidental character was discovered.

The *double entendre* of the pun illustrates the rivalry of habit or apperceptive systems on a slightly higher level. The pun probably originated in a lapse. A cat chasing its tail is jumping at conclusions, to be sure, but since the fore-part of the sentence arouses one apperceptive system and the latter part such a very different one, a momentary conflict is set up which resolves itself agreeably as we at once vindicate the cat and save our logical terminology. The same general principle may be recognized as operative in the reply of David Garrick (who was small of stature) to a woman who had expressed the wish that the great actor were taller, that he might appear to better advantage on the stage: "Truly, madam," he said, "I am sorry that I cannot stand higher in your estimation."

Most theories of the ludicrous have dwelt on the factor of incongruity. And this is doubtless indispensable in order to bring about the tension whose sudden resolution results in the irradiation prerequisite to the pleasurable emotion. But equally important and even more fundamental is the factor of congruity. Mere surprise, abrupt transition or sudden movements will make the child laugh, but such cacchination is scarcely to be regarded as expressing an appreciation of the comic, being probably a



mere reflex. It is essential to the comic as an æsthetic experience that there shall be a more or less elaborate background of meanings the disturbance of which is the condition of the incongruity. The situation must first mean something before the irruption of a new meaning can create a conflict of meanings. Sully quotes Rolfe as saying that "he who cannot enjoy nonsense must be lacking in sense." Nonsense is not no-sense but a category of sense. This raises a question the satisfactory answer to which would involve a nice piece of discriminative introspection and analysis: How irrelevant may the details of a situation be and yet be ridiculous, or, in other words, what are the limits within which congruity and incongruity are comic?

It is evident that the disadaptation or incongruity must not be too great nor involve serious practical or ethical consequences, and, on the other hand, the unexpected congruity must not be too relevant or morally significant. When, with Lord Raleigh, we call the negro 'God's image carved in ebony,' the felicitous expression, for many persons with vivid memories of the issues of a bloody war, is too significant to be comic. So with Thomas More's remark to the headsman, that although he required assistance in mounting the scaffold, he would shift for himself in coming down. One may smile only when he is able to abstract from the actualities of the situation.

Discrepancies on the moral side are less easily interpreted. The fact of inconsistency with some ethical standard may have something to do with the tensional phase, and possibly the consciousness of superiority in the subject may explain the pleasure gotten from contemplating moral delinquency on somewhat the same principle that the savage and the child laugh at physical deformities and the uncultivated laugh at a stupid remark. But probably the comic element in the immoral lies in the momentary relapse of the conventional mind to some more primitive type of reaction. This is certainly borne out by the jokes on sacred subjects, the coarse jokes of the 'virile' man in uncovering sexual matters, etc. Many writers have hinted that there is something essentially immoral in the comic. Charles Lamb suggests that a leading element in the enjoyment of certain forms of comedy consists in the fact that they free us

from the burden of our habitual moral consciousness. Aristotle says that Comedy is "an imitation of characters of a lower type, and that the ludicrous is a subdivision of the Ugly, consisting in some defect or ugliness which is not painful or destructive." The Fuegians, says Sully, laughed uproariously at a white man's washing his face, and adds that the comic involves the "presentation of something in the nature of a defect, a failure to satisfy some standard requirement, as that of law or custom, provided that it is small enough to be viewed as a harmless plaything," such, for example, as failure to comply with a social convention, yet "so trifling that we do not feel called upon to judge the short-coming severely." This theory of degradation or principle of lowered dignity is often called the moral theory of the comic, since it holds that there is something unworthy or mean and contemptible in the ludicrous object.

### III. 'DISCHARGE.'

The irradiation or discharge and subsequent recovery of poise as we free ourselves is due to the relatively abrupt coalescence and reinforcement of habit systems. The relative unexpectedness, abruptness, suddenness, surprise of the final resolution of tension has been dwelt upon in all attempts to analyze the comic situation. The ludicrous, as one writer has phrased it, is "an approximately instantaneous revelation of an incongruous congruity." Laughter, says Kant, "is an affection arising from the sudden transformation of a strained expectation into nothing."

In the more elementary forms of the comic the irradiation or discharge is of an abrupt and even explosive character. As has been said, mere abruptness of transition in itself, such as a sudden or jerky movement, is sufficient to make a young child laugh, and this is part of the stock-in-trade of the professional clown. It is true also that "brevity is the soul of wit." "When we hold our breath in expectation, and then undergo a violent change of tension through the expectation coming to nothing," says Bosanquet, "we certainly go through a process like that which Kant describes" in his famous definition of laughter.

*Abruptness in the cruder sense is not however indispensable,*

as is evident from those types of the comic in which one feels suffused with the humor of a situation or quietly wrapped in the pleasurable moment rather than swept away or convulsed by it. Thus the story is told of a lad who was kept after school and punished by being required to write an essay of one hundred words. He wrote the following composition: "Jack went to the door and called 'Kitty, kitty, kitty, kitty' . . . (ninety-three times)." All that seems necessary in many instances is the establishment of a connection, involving not too many transitions, with some apperceptive system which has pleasant associations, as, for example, when the patriotic geographer said that America was bounded on the North by the Aurora borealis, on the South by the torrid zone, on the East by the history of the past, and on the West by the Day of Judgment.

The factor of surprise in the emotion of the ludicrous is the emotional counterpart of the fact of irradiation. The relief of the laugh and the repose of the smile illustrate the effects of the pleasurable diffusion of accumulated energies. In ordinary cases of the comic, as Sully says, "the disagreeable feeling of disturbance begins at once to give way to a pleasurable form of consciousness," the whole experience being best described as a "transition (or rapid series of transitions) from a feeling of confinement or contraction to one of liberation or expansion."

#### IV. 'RECOVERY OF POISE.'

The prerequisite of the comic is that we shall be left in control of the situation; as Miss Puffer says, there 'must be a way out.' In accordance with the theory of emotion which has been set forth, the comic is connected with the fact of relatively abrupt relief from the tension which accompanies the vigorous exploiting of habit-systems. The smile, Sully remarks, is the position of the features when food is presented, the laugh being merely the smile combined with the *eh* sound which expresses relief from tension. The sense of relief comes from the fact that the apparent break or disadaptation suddenly turns out to be really an adaptation. Its pleasurable character is due to the fact that the summated energies of the organism are thereby enabled to irradiate along familiar lines before the inhibition

has been carried to the point of pain. The comic thus describes the same situation that, beyond a certain point, would be called tragic. As Horace Walpole put it, life is a comedy to him who thinks, but a tragedy to him who feels.

The consciousness of superiority or sudden glory in the subject, of which Hobbes speaks, 'taking somebody down,' or, what is the counterpart of this, the sense of inferiority or degradation in the object, would seem to be but clumsy ways of expressing this fact of control. Bain follows him in defining the occasion of the ludicrous as "the degradation of some person or interest possessing dignity in circumstances that excite no other strong emotion." Lipps says that in the incongruity there must be a belittling presentation. Carlyle in milder terms speaks of a sense of the ridiculous as a brotherly sympathy with the downward side. "Twenty-seven millions, mostly fools. Well, better fools than knaves."

The savage laughs at the sight of a man chastising his wife. We laugh at the little mischance of capsized pompousness, at inflated pride followed by a fall. We smile at the embarrassed youth, the bashful maiden, the naïveté of the child. We laugh at the German, mentioned by Sully, who when asked how his wife was, said: "She is generally lying, and when she is not lying she is swindling," meaning to say 'lying down' and 'feeling giddy' (hat Schwindel),—and at the child who when asked, "Isn't Grandpa very kind to play with you, Dear?" replied: "I'm playing with *him*,"—and at the old lady who said she could grasp the fact of the astronomers' finding new stars with their improved telescopes, but what she could not understand was how they discovered their names.

The ludicrous character of the grotesque, burlesque, satire, ridicule, the lampoon, and the practical joke all lend themselves to this interpretation. Humor and wit are less easily explained. And, yet, it is believed that a careful analysis will show that the principle holds here as well, as disclosed by the social value of a sense of humor. Laughter because of its contagiousness and its effect in cementing social distinctions is one of the most powerful factors in developing a sense of social solidarity in the individual. Ridicule is one of the instruments evolved by the

group for the socializing of its recalcitrant members : it rounds up the mavericks of the human herd — the prigs and pedants, cranks and dreamers, the absent-minded man, the man-of-one-idea, and the man-behind-the-times.

“Anything in the shape of a feeling of inferiority to, or even of respect for, the laughable person inhibits the laughter of the contemplator,” says Sully, or “if a person finds himself distinctly involved in a disgrace, the absurd situation, or whatever else provokes laughter, he no longer laughs, or laughs in another key. I see my estimable fellow-pedestrian lose his hat at a street corner where the wind lies in ambush : my soul expands exultingly. The moment after, I, too, may fall a victim to the ambushade, in which case I probably stop laughing and become the subject of a different emotion.” And another fact is still more decisive. “If no superiority is implied in our common laughter at others, how does it come about that we all have so very obstinate a dislike to being made its object?”

#### V. THE COMIC AND THE TRAGIC.

This suggests, in conclusion, that laughter is the most primitive æsthetic experience. To weep, it might be said, is equally elemental, but, save in Tragedy, it is not æsthetic, whereas the roots of the comic reach far back of the origins of Comedy. The essence of the æsthetic experience is a stimulating repose, just what we find, for the first time clearly, in primitive laughter — a rudiment upon which Tragedy is grafted at a comparatively late date by a curious process of inversion.

The primitive hunt dance, an extemporaneous celebration, perhaps, of some recent triumph, is only an elaboration of the sigh of relief and the consciousness of success which follow upon any strenuous activity. The more or less detail of stimulating ideational content which accompanies this emotional overflow is naturally derived from the serious occupation which has been thus brought to a satisfactory culmination. This content, if it serves to sustain and control such an emotional outburst, is in a fair way to become æsthetic, inevitably first taking the form of Comedy, since it is a universal law of emotion to expand pleasurable and restrict painful fields of consciousness. That

this would be so is borne out by the fact that in early pantomime the portrayal of tragic situations often passes insensibly into real tragedy, a mock war dance, for example, ending in a bloody conflict.

The tragic, while doubtless present to the fullest extent as a part of the material represented in such a dance, is there however not to excite a vicarious pity and fear, in the sense which Aristotle attached to these words, but simply as a feature of the revived content thrown into the central glow of the dramatic representation in so far as it contributes to the enhancement of the total pleasurable recall. It would seem to require a considerable advance in sophistication and self-analysis before the tragic emotions might be held in suspense on the attenuated thread of conscious self-illusion, and the development of a high level of inhibition and self-control before a vicarious might supplant the immediate response to so strong a stimulus. The comic, therefore, as a form of art, would appear to antedate the tragic, however intimately they may be originally associated in the direct experience.

# THE PSYCHOLOGICAL REVIEW.

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## THE INFLUENCE OF TEMPERATURE AND THE ELECTRIC CURRENT ON THE SENSIBILITY OF THE SKIN.<sup>1</sup>

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### I. INTRODUCTORY.

Within the last few years the progress in physical chemistry has given an insight into physiological processes which before were not even amenable to an exact investigation. On becoming familiar with this movement at Professor Loeb's laboratory at the University of California, it occurred to me that it might be useful to apply the physico-chemical methods to psychophysics and investigate from a new point of view the relation between the stimulus and the sensation.

It is at least possible that the physiological process of sensation may depend on the velocity of a definite chemical reaction. If so, it would in some way vary with the temperature in accordance with the van't Hoff-Arrhenius law. My first attempt to investigate this problem was made with sensations of taste. No definite results were obtained. It was, however, proved by Weber in 1847 that temperature does affect the sensation of taste and that there are limits of temperature beyond which taste is impossible.<sup>2</sup> Between these limits it should vary according to a definite law, but its investigation would be involved in difficulties. At Professor Loeb's suggestion I left the problem of taste for that of touch. I made some preliminary experiments on the touch threshold at Berkeley.

<sup>1</sup> From the psychological laboratory of the Catholic University of America.

<sup>2</sup> 'Ueber den Einfluss der Erwärmung und Erkaltung der Nerven auf ihr Leitungsvermögen' (Müller's), *Archiv für Anat. und Physiol. und wiss. Med.*, 1847, 2d pt., pp. 342-356.

These resulted in showing that the touch threshold varies with the temperature of the skin. The work has been continued at the Catholic University of America in Washington, under the direction of Doctor Pace, to whom I am thankful for many suggestions and for constant assistance. Among others, I am especially indebted to my subjects, Mr. Bour (B.), Mr. Ferris (Fe.), Mr. Finnegan (F.), and Mr. Hoey (H.).

## II. THE METHOD OF VARYING THE TEMPERATURE OF THE SKIN.

To vary the temperature of the skin I placed over the area investigated—the upper portion of the inner side of the right forearm—a hot water bag filled with water of a known temperature. The temperature of the skin thus produced is not that of the water in the bag, nor will it be that of a thermometer between the arm and the bag, but either less or greater than that of the thermometer according as the bag is above or below the physiological zero. The thermometer approximates the arithmetical mean between the temperature of the bag and that of the arm. A thermometer with cylindrical bulb placed between two hot water bags varies from the arithmetical mean in opposite directions according as the hot or the cold bag is on top. The mean of two such readings is not far from the arithmetical mean of the temperature in the two hot water bags. It is always a little above—a phenomenon probably due to conduction currents which are faster in the warm than in the cold water. If the conductivity of the skin approximates that of rubber—and both are poor conductors—the temperature of the thermometer will approximate the arithmetical mean between the skin temperature and that of the bag. It can thus be calculated with a degree of accuracy that will be greater in the region of the physiological zero and will decrease on either side of that zero.

## III. INFLUENCE OF THE TEMPERATURE OF THE SKIN ON THE SPATIAL THRESHOLD.

In investigating the 'spatial' threshold I used the usual bar compass, but to the customary points I attached two horse hairs of equal length. The diameters of the cross section of the top of each hair were  $.24 \times .22$  mm. measured by the microscope.



The bending power varies according to the strength of the stroke and the extent to which the hair is bent. The bending power of one was between 8.3 and 7.0 gr., that of the other between 8.7 and 8.0 gr. In giving the stimulus the attempt was made to acquire a stroke of constant pressure. The flexibility of the hairs makes this easier than when compasses with stiff arms are used.

The area measured was the upper portion of the inner forearm. Only the transverse threshold was measured. The arm was cooled or warmed by water contained in an ordinary hot water bag. This bag, of course, had to be raised from the arm in order to give the stimulus, but was immediately replaced. As the temperature of the bag differs from that of the room, there is some source of error due to the temporary removal of the bag.

Below are given the results for one subject. Under  $\Delta$  is given the threshold distance between the two points at which they were

Temperature of Bag.		Temperature of Thermometer.		Temperature of Arm (Calculated) <sup>1</sup>	$\Delta$	
Degrees.	Mean Value.	Degrees.	Mean Value.	Degrees.		Mean Value.
50	50	44	44	38	5.1 cm.	5.1
45		41			4.1	
45	45	40	40.5	36	3.6	3.8
37		36.2			3.5	
37	37	36.2	36.2	35.4	3.3	3.4
33		33			3.9	
32.5		33.5			3.8	
32	32.5	33	33.2	33.9	3.4	3.7
21.5		25.5			3.7	
20	20.7	23.6	24.2	27.7	4.1	3.9
10	10	17	17	24		4.6
5	5	13	13	21		(6.0) <sup>2</sup>

<sup>1</sup> Cf., p. 356.

<sup>2</sup> The value given in brackets is not the mean of this series but the maximum. The entire series was as follows:

Down.		Up.	
(1)	6.0	(2)	5.2
(3)	5.0	(4)	4.8
(5)	4.4	(6)	4.2

I attribute this constant fall to the effect of warming as I removed the bag to measure the threshold. A control experiment with an ice bag (thermometer between bag and arm 2.1°) gave a threshold beyond 7 cm.

felt as a single point. Each value of  $\Delta$  is a mean taken from six experiments (occasionally only four). Three of these values were taken by reducing the distance between the points until they were felt as one, and then by increasing them from below the threshold until they were felt as two. On plotting the curve one will find that it apparently has a minimum at  $35.4^{\circ}$  C. which is produced by water at  $37.0^{\circ}$  C. Beyond this point it rises much more abruptly than it fell on the other side.

The very steep slope on the side of the curve above the minimum may not be due solely to a decrease in sensibility. The subject found the  $50^{\circ}$  bag very painful, and each stimulus was like two needles going deep into the skin. Consequently it was harder to attend to the character of the stimulus and more difficult to recognize the points as single or double.

To what, we may ask, is the influence of temperature on the spatial threshold due? The answer to this is that temperature varies the sensibility of the skin. Consequently, it weakens or strengthens the stimulus. Weak stimuli are less, and strong more readily perceived as double. Therefore the variation of the spatial threshold with temperature expresses the variation in the strength of the stimulus due to the temperature of the skin. This conclusion rests upon two statements: (*a*) The spatial threshold is a function of the intensity of the stimulus; (*b*) the touch threshold is a function of temperature.

The following sections will make these points clear and also show that the touch threshold varies in the same way as the spatial threshold with temperature.

#### IV. INFLUENCE OF THE PRESSURE OF THE POINTS ON THE SPATIAL THRESHOLD.

In order to investigate the influence of the pressure of the points on the spatial threshold I took an *æsthesiometer* to which I attached much weaker bristles than those above described. The experiments given below were made on the inner side of the first phalanx of the middle finger. Those quoted are for subject H. The values given represent millimeters.

In judging of the value of these results one may use the following method: Let us suppose that the experiments with

NORMAL (*i. e.*, WITHOUT BAG ON THE HAND).

STRONG.		WEAK.	
Down.	Up.	Down.	Up.
3	4	4.5	5
2	3	3.0	5
5	7	7.5	10
Mean..... 3.0 mm.		Mean..... 4.4 mm.	

## BAG ON HAND CONTAINING WATER AT 30°.

STRONG.		WEAK.	
Down.	Up.	Down.	Up.
4	3	5	6
3	3	4	6
7	6	9	12
Mean..... 3.2 mm.		Mean..... 5.2 mm.	

## BAG ON HAND CONTAINING WATER AT 50°.

STRONG.		WEAK.	
Down.	Up.	Down.	Up.
2	5	4	7
3	4	5	6
3	4	5	6
8	13	14	19
Mean..... 3.5 mm.		Mean..... 5.5 mm.	

weak bristles constitute a kind of normal with which the observations with strong bristles are to be compared. In any set of experiments the individual readings for strong bristles would then by the law of probabilities exceed and fall short of the mean for weak bristles an equal number of times. The probability of one excess of the weak over the strong would be  $\frac{1}{2}$ : for two successive excesses  $\frac{1}{4}$  etc. In the 14 experiments given all of the individual readings for the strong are below the means for the weak. The probability of this happening by chance is  $1/2^{14} = 1/16384$ .

An apparent difficulty is presented by the fact that the individual readings are of two kinds "up" and "down." But while the "downs" have a tendency to fall short of the mean this is counterbalanced by an opposite tendency of the "ups" to exceed the mean. There is therefore very strong evidence to show that the spatial threshold is a function of the pressure of the stimulus. Just what law it follows I have not investigated.

## V. INFLUENCE OF THE TEMPERATURE OF THE SKIN ON THE TOUCH THRESHOLD.

The instruments used in investigating the touch threshold were a set of von Frey æsthesiometers with sliding tubes. These I carefully graduated on a delicate balance. Practice in graduating the hairs is, I think, an indispensable condition for accurately using them in the actual experiments. The first trials at the balance seem hopeless, and if one touched the skin in the same way as he is likely at first to touch the pan of the balance, no constant results could be obtained. By acquiring the proper delicacy of touch in graduating the hairs it becomes possible to perform accurate experiments.

The diameter of the hairs was measured by means of a microscope. Below are given the tables of standardization for the æsthesiometers most frequently used. The measurement of the bending power is one of great difficulty. Care must be taken not to breathe on the pans of the balance. Some sort of screen must be put between the face and the balance while measuring the bending power. With thin long hairs the minimum bending power is indicated by a very slight movement, which is scarcely perceptible. For stiffer hairs one is aided by a slight sound of the weight pan falling back into place. Only the medium lengths of the hairs give accurate measurements.

### ÆSTHESIOMETER I.

$$\begin{array}{ll} \text{Diameter} = \begin{array}{l} .115 \text{ mm.} \\ .090 \text{ mm.} \end{array} & \text{Radius} = \begin{array}{l} .0575 \text{ mm.} \\ .0450 \text{ mm.} \\ 2 \overline{) .1025 \text{ mm.}} \\ \text{Mean radius} = .052 \text{ mm.} \end{array} \end{array}$$

Scale Reading.	Bending Power.	Bending Power Divided by Mean Radius.	Scale Reading.	Bending Power.	Bending Power Divided by Mean Radius.
20	0.006 <sup>1</sup> -7	0.115 gm.	32	0.009-10	0.192 gm.
25	0.006-7	0.135 gm.	33	0.011-12	0.231 gm.
26	—	(0.137 gm.) <sup>2</sup>	34	0.018-19	0.366 gm.
27	—	(0.140 gm.)	35	0.023-24	0.452 gm.
28	—	(0.142 gm.)	36	0.031-32	0.606 gm.
29	—	(0.148 gm.)	37	0.050-51	0.980 gm.
30	0.007-8	0.154 gm.	38	0.070-80	1.539 gm.
31	0.008-9	0.173 gm.			

<sup>1</sup> Numbers in italics indicate the bending power to which the hair more nearly approaches.

<sup>2</sup> Values given in parentheses are obtained by plotting a curve, the scale reading giving the abscissas, and the bending power, or bending power divided by mean radius, the ordinates.

ÆSTHESIOMETER III<sup>a</sup>.

Diameter = .150 mm.  
.110 mm.

Radius = .075 mm.  
.055 mm.

2  $\overline{.130 \text{ mm.}}$

Mean Radius = .065 mm.

Scale Reading.	Bending Power.	Bending Power Divided by Mean Radius.	Scale Reading.	Bending Power.	Bending Power Divided by Mean Radius.
0	0.040-42	0.601 gm.	25	0.075-80	1.154 gm.
5	0.045	0.692 gm.	28	(0.084)	(1.291 gm.)
10	(0.052)	(0.800 gm.)	30	0.090-95	1.461 gm.
15	0.059-60	0.907 gm.	31	0.110-115	1.769 gm.
20	(0.67)	(1.030 gm.)	32	0.135-145	2.230 gm.
22	(0.070)	(1.077 gm.)	33	0.175-180	2.769 gm.
23	(0.072)	(1.110 gm.)	34 <sup>1</sup>	0.230-235	3.615 gm.
24	(0.074)	(1.138 gm.)			

<sup>1</sup> Beyond this point the hair could not be accurately calibrated.

ÆSTHESIOMETER III<sup>a</sup>.

Diameter = .1805 mm.  
.1881 mm.

Radius = .09025 mm.  
.09405 mm.

2  $\overline{.18430 \text{ mm.}}$

Mean radius = .09215 mm.

Scale Reading.	Bending Power.	Bending Power Divided by Mean Radius.	Scale Reading.	Bending Power.	Bending Power Divided by Mean Radius.
0	0.035-40	0.434 gm.	15	0.130-135	1.466 gm.
5	0.050-55	0.597 gm.	20	0.225-230	2.497 gm.
10	0.080-85	0.923 gm.	25	0.640-650	7.106 gm.

## ÆSTHESIOMETER IV.

Diameter = 0.328 mm.  
0.200 mm.

Radius = 0.164 mm.  
0.100 mm.

2  $\overline{0.264 \text{ mm.}}$

Mean radius = 0.132 mm.

Scale Reading.	Bending Power.	Bending Power Divided by Mean Radius.	Scale Reading.	Bending Power.	Bending Power Divided by Mean Radius.
0	0.090-95	0.689 gm.	25	(1.55)	(11.7) gm.
5	0.135-140	1.061 gm.	27	(3.00)	(22.8) gm.
10	0.275-280	2.121 gm.	30	3.250-3.300	25.000 gm.
15	0.440-450	3.401 gm.	40	Beyond 10.0 gr.	Beyond 75 gm.
20	0.820-830	6.280 gm.			

The method of cooling and heating the skin employed in the previous experiments was used in this. The inner fore-arm was the place experimented on. A touch spot was sought in this locality and marked with a capillary glass tube containing a solution of methylene blue.

Below are given the laboratory notes for the experiments with one subject. They are followed by a table of maximum values for the thresholds at the various recorded temperatures. The maximum values are preferable because of the error that is essential to the method I employed. In lifting the bag to take the threshold the skin cools off or warms up a little before the actual measurement can be taken. Consequently the measurements in general are a little below what they should be.

## SUBJECT F: 13 Jan., 1910.

Normal threshold æs. I. .35-.36 = 0.606-0.980 gm.

3:47 p. m. Bag 50° placed on arm.

3:52-3:56 Threshold æs. I. 39-40 = beyond 1.5 gm.

3:59 Bag cooled to 46°. Replaced by another 50°.

4:02-4:05 Threshold æs. III<sup>a</sup> 20-25 = 1.030-1.154 gm.

4:10 Bag replaced by another 50°.

4:12-4:14½ Threshold æs. III<sup>a</sup> 22-24 = 1.077-1.138 gm.

4:15 Bag removed and not replaced.

4:20-4:23 Attempted measurement with æs. III<sup>a</sup> fails. Threshold æs. I. 35-36 = 0.606-0.980.

4:25-4:27 Threshold æs. I. 35-36 = 0.606-0.980 gm.

## SUBJECT F: 14 Jan., 1910.

Normal threshold æs. I. 32<sup>1</sup>-33 = 0.192-0.231 gm.

3:25 p. m. ICE BAG placed on arm.

3:30-3:32 Attempted measurements with æs. III<sup>a</sup>, doubtful at 40°.

3:35-3:40 Attempted measurement with æs. IV, subject felt hard pressure at 40 which is beyond 75.0 gm.

3:41 Ice bag removed and not replaced.

3:42-3:42½ Æs. III<sup>a</sup> 30-35 = 1.461-beyond 3.6 gm.

3:43-3:43½ Æs. III<sup>a</sup> 5-15 = 0.692-0.907 gm.

3:44-3:45 Æs. I 35-37 = 0.452-0.980 gm.

3:46-3:46½ Æs. I 34-35 = 0.366-0.452 gm.

3:48-3:48½ Æs. I 33-34 = 0.231-0.366 gm.

3:50-3:52 Æs. I 36-37 = 0.606-0.980 gm.

3:55-3:55½ Æs. I 36-37 = 0.606-0.980 gm.

3:56-3:58 Æs. I 36-37 = 0.606-0.980 gm.

## SUBJECT F: 14 Feb., 1910.

Bag 10° placed on arm and replaced several times by a fresh bag 10° during 10 minutes. The temperature registered by a thermometer placed between bag and arm was 15.4°. Threshold measured at end of the 10 minutes was æs. III<sup>a</sup> 33-34 = 2.769-3.615 gm.

3:35 p. m. Bag warmed to 11.2°; replaced by another 10°.

<sup>1</sup> Values in italics mean that the threshold is nearer that limit than the other. This can sometimes be judged by the number of correct guesses or by the subject saying that the sensation perceived was very clear or scarcely noticeable.

3:39	Threshold æs. III <sup>a</sup> 30-31 = 1.461-1.769 gm.
3:41	Bag warmed to 11.2°; replaced by another 10°.
3:44	Threshold æs. III <sup>a</sup> 32-33 = 2.230-2.769 gm.
3:48	Bag warmed to 11.2°; replaced by another 10°.
3:53	Threshold æs. III <sup>a</sup> 31-32 = 1.769-2.230 gm.
3:53	Bag removed and not replaced.
4:07	Threshold æs. I. 31-32 = 0.173-0.192 gm.

## SUBJECT F: 10 Mar., 1910.

Normal threshold æs. I. 33-34 = 0.231-0.366 gm.

3:34 p. m.	Bag 33.5° placed on arm.
3:50-4:00	Threshold æs. I. 35-36 = 0.452-0.606 gm. Threshold æs. III <sup>a</sup> 0-5 = 0.601-0.692 gm. Temperature of bag at end of experiment 31.7°. Temperature between bag and arm at end of experiment 32.6°.
4:20	Bag 50° placed on arm.
4:38	Threshold æs. III <sup>a</sup> 28-30 = 1.291-1.461 gm. Temperature of bag at end of experiment 46.8°. Temperature between bag and arm at end of experiment 42.9°.

## SUBJECT F: 15 Mar., 1910.

Normal threshold æs. I. 33-34 = 0.231-0.366 gm.

2:16 p. m.	Bag 33° placed on arm.
2:25	Bag cooled to 32.1°; replaced by another 33°.
2:30	Threshold æs. I. 34-35 = 0.366-0.452 gm.
2:35	Temperature between bag and arm 33.6°, temperature of bag 32.3°.
2:35½	Bag 25° placed on arm.
2:42	Bag cooled to 24.8°; replaced by another 25°.
2:46	Threshold æs. I. 35-36 = 0.452-0.606 gm.
2:53½	Threshold æs. III <sup>a</sup> 25-30 = 1.154-1.461 gm.
2:57	Temperature between bag and arm 28.4°, temperature of bag 24.6°.
2:58	Bag replaced by another 24.6°.
3:00	Threshold æs. III <sup>a</sup> 25-30 = 1.154-1.461 gm.
3:04	Temperature between bag and arm 28.2°, temperature of bag 24.6°.

## SUBJECT F: 22 Mar., 1910.

Normal threshold æs. I. 35-36 = 0.452-0.606 gm.

2:16 p. m.	Bag 34° placed on arm.
2:21½	Bag cooled to 32.2°; replaced by another 34°.
2:24	Threshold æs. I. 35-36 = 0.452-0.606 gm.
2:28	Temperature between bag and arm 34.5°, temperature of bag 33.4°.
2:29	Bag 37° placed on arm.
2:32	Bag cooled to 36.4°; replaced by another 37°.
2:34½	Threshold æs. I. 34-35 = 0.366-0.452 gm.
2:38	Bag cooled to 36.4°; replaced by another 37°.
2:42½	Threshold æs. I. 33-34 = 0.231-0.366 gm.
2:45	Temperature between bag and arm 36°, temperature of bag 36.5°.
2:48½	Threshold æs. I. 34-35 = 0.366-0.452 gm.
2:50	Temperature between bag and arm 36.2°, temperature of bag 36.6°.
2:51½	Bag 40° placed on arm.

2:57	Bag cooled to 39.3°; replaced by another 40°.
3:00	Threshold æs. I. 33-34 = 0.231-0.366 gm. <sup>1</sup>
3:03½	Bag cooled to 39.0°; replaced by another 40°.
3:08½	Threshold æs. I. 34-35 = 0.366-0.452 gm.
3:11½	Temperature between bag and arm 37.8°, temperature of bag 39.1°.
3:12	Bag 45° placed on arm.
3:16	Bag replaced by another 45°.
3:20	Threshold æs. I. 35-36 = 0.452-0.606 gm.
3:25	Bag cooled to 43.1°; replaced by another 45°.
3:30	Threshold æs. I. 34-35 = 0.366-0.452 gm.
3:33½	Temperature between bag and arm 40.2°, temperature of bag 43.2°.
3:34	Bag 50° placed on arm.
3:40	Bag replaced by another 50°.
3:42½	Did not feel æs. I. 36 = 0.606 gm.
3:49	Bag cooled to 48°; replaced by another 50°.
3:54	Threshold æs. III° 20-25 = 2.497-7.106 gm.
3:58	Temperature between bag and arm 42.5°, temperature of bag 48.2°.
4:01	Threshold æs. III° 20-25 = 2.497-7.106 gm.
4:04	Bag 34° placed on arm.
4:10	Threshold æs. III° 0-5 = 0.434-0.597 gm.
4:13	Bag cooled to 33.4°; replaced by another 34°.
	Threshold æs. I. does not feel 37 = 0.980 gm.
	Threshold æs. III° 15-20 = 1.466-2.497 gm.

TABLE OF MAXIMUM VALUES.

Temperature of Bag. Degrees.	Temperature Between Bag and Arm. Degrees.	Calculated Temperature of Arm. Degrees.	Maximum Threshold.
0	—	—	Beyond 75 gm.
10	15.4	20.8	3.615 gm.
24.6	28.4	32.2	1.461 gm.
33.4	34.5	35.6	0.606 gm.
36.6	36.2	35.8	0.452 gm.
39.1	37.8	36.5	0.452 gm.
43.2	40.2	37.2	0.606 gm.
46.8	42.9	39.0	1.461 gm.

The assumption I have made in calculating the temperature of the arm is that the temperature of a thermometer placed between the skin and a hot water bag approximates the arithmetical mean between skin temperature and that of the hot water bag. In the region of the minimum threshold the error of this assumption can scarcely exceed 1° centigrade. There are not wanting experiments to test the accuracy of this assumption M. J. Lefèvre by means of a specially constructed thermopile measured the temperature of the skin two millimeters below

<sup>1</sup> Felt a touch once only at 33, always at 34.



the surface, and also the surface temperature of the skin in baths of 5°, 12°, 18°, 24° and 30° C. If his results are correct, in calculating his observed temperature of the skin below the surface from the observed surface temperature and that of the bath, my assumption would lead to the following errors:

5° C. Minus.	12° C. Minus.	18° C. Minus.	24° C. Minus.	30° C. Minus.
-3.6° C.	-3.1° C.	-2.1° C.	-1.1° C.	-0.5° C.

Since, however, it appears that Lefèvre measured his temperatures in all cases after a constant interval of time, and since the longer the arm stays in the cold bath the lower will be its temperature until an equilibrium is established, there is an increasing error in his results as he goes away from physiological zero. This being taken into consideration, the assumption I have made cannot be far away from the reality.<sup>1</sup>

In spite of the rough character of these experiments the numerical data are not without value. They afford us some very definite information about the influence of temperature on sensibility. From zero on to a certain point, which I have not exactly determined, sensibility increases with the rise of temperature. Beyond this point it decreases. The temperatures of the bag on either side of this maximum were 33.4° and 43.2°. The readings of a thermometer placed between the bag and the arm on either side of this maximum were 34.5° and 40.2°. It is clear therefore that the maximum lies above 34.5° and below 40.2°. If we assume that the temperature of the thermometer between the bag and the arm is the arithmetical mean between the temperatures of the bag and the arm, then the point of maximum sensibility lies between 35.8° and 37.2°. Allowing for an error of about 8° centigrade on either side we may say with fair probability that the temperature of maximum sensibility lies between 35° and 38° C. This result that the sensibility of the skin increases with rising temperature to a maximum and then decreases, has been confirmed with other

<sup>1</sup> Cf. M. J. Lefèvre, 'Étude expérimentale du pouvoir protecteur de la peau et des ses coefficients de la conductibilité,' *Journal de physiologie et de pathologie générale*, 1901, III., pp. 1-14.

subjects. Von Frey<sup>1</sup> failed to find that cooling had any influence on the sensibility of the skin. His method of cooling however was the use of a water bath at 8° C. It is rather remarkable that he did not and his failure must have been due to some oversight. The water bath however is a bad method of cooling because it seriously affects the condition of the skin.

#### VI. INFLUENCE OF THE DIRECT CURRENT ON THE SENSIBILITY OF THE SKIN.

As early as 1863 Fräulein Nadjeschda Suslowa found that the sensibility of the skin was increased in the neighborhood of the kathode and decreased around the anode. Professor Setchenow, however, in communicating her results to the *Zeitschrift für rationelle Medicin*, added a note in which he said that he had repeated his student's work but could obtain no definite results. He thought, however, that this was due to his lack of experience in this line of work, and left the matter entirely to the trustworthiness of Fräulein Suslowa's work. A complete analysis of Fräulein Suslowa's results is given below (pp. 365 ff.). I have repeated her experiments with some modifications.

Robert Graeber<sup>2</sup> confirmed the results of Fräulein Suslowa for the spatial threshold. He refers to the writings of other authors on this problem, but abstains from giving any references beyond their names. Graeber rejected Nadjeschda Suslowa's method of experimenting on the basis of variable results obtained by Carl Spanke. Spanke found that on applying two electrodes to the skin he would at times get the results found by Fräulein Suslowa, but then again he would get a decrease of sensibility at both electrodes, and again an increase of sensibility at both electrodes. In the beginning I was annoyed by the same fluctuations, but I believe now that I can obtain Fräulein Suslowa's results at any time by using the proper strength of current. Just as in the motor nerves so also in the skin with weak currents the cathodic effect extends over to the anode, with strong currents the anodic effect extends over to the cath-

<sup>1</sup> 'Untersuchungen über die Sinnesfunctionen der mensch. Haut,' *Abh. der M. phys. Classe der K. S. Gesellschaft der Wiss.*, XXIII., p. 221.

<sup>2</sup> 'Untersuchungen über den Einfluss galvanischer Ströme auf den Tastsinn der Haut' (Diss., Bonn, 1884, pp. 21).

ode. If one bears that law in mind and also remembers that the electrolysis of skin produces at the anode a substance that is not readily absorbed, he will have no difficulty in verifying Fräulein Suslowa's law of the electrotonus of the skin.

Graeber obtained constant results by placing the arm in a water bath of  $27^{\circ}$  R. One electrode was placed on the chest, the other in the water bath. The essential feature of this procedure really consisted in nothing more than a weakening of the current by the introduction of considerable resistance. Graeber also found as a regular occurrence the phenomenon of 'transfer.' If the sensibility on one arm is increased by the electric current it is simultaneously decreased on the other arm to which no current has been applied, and *vice versa*.

In my experiments I used for zinc bracelets flexible sheet zinc about 1 cm. in width, hinged in the middle and capable of being adjusted to the size of the arm by a set screw. Between this and the arm I wrapped a strip of cotton felt soaked in ordinary tap water or zinc sulphate solution. After placing the bracelets on the arm I found inside of them (*i. e.*, in the path of the current) two touch spots as close as possible to the bracelets. I then measured the thresholds of these spots, after which I turned on the current. The following results were obtained :

SUBJECT B, 31 March, 1910.

Two Edison-Laclede cells.

About 5 cm. between electrodes.

*Upper Electrode.*

*Lower Electrode.*

Threshold before current was turned on :

Æs. I. 32-33 = 0.191-0.231

Æs. I. 33-34 = 0.231-0.366

Threshold after current was turned on :

+  
Æs. I. 33-34 = 0.231-0.366

—  
Æs. I. 32-33 = 0.191-0.231

Current reversed :

—  
Æs. I. 32-33 = 0.192-0.231  
Æs. I. 32-33 = 0.192-0.231

+  
Æs. I. 32-33 = 0.191-0.231  
Æs. I. 32-33 = 0.191-0.231

Current reversed :

+  
Æs. I. 33-34 = 0.231-0.366

—  
Æs. I. 31-32 = 0.173-0.192

SUBJECT B, 13 May, 1910.

Threshold before current was turned on :

Æs. I. 34-35 = 0.366-0.452

Æs. I. 33-34 = 0.231-0.366

## Threshold after current was turned on :

+	—
Æs. I. 32-33 = 0.366-0.452	Æs. I. 32-33 = 0.192-0.231
32-33 = 0.366-0.452	30-31 = 0.154-0.173
32-33 = 0.366-0.452	29-30 = 0.148-0.154
32-33 = 0.366-0.452 <sup>1</sup>	

## Current reversed :

—	+
Æs. I. 30-31 = 0.154-0.173	Æs. I. 29-30 = 0.148-0.154
Æs. I. 31-32 = 0.173-0.192	Æs. I. 31-32 = 0.173-0.192
	Æs. I. 32-33 = 0.192-0.231

## Current broken for five minutes :

## Threshold at end of five minutes :

Æs. I. 34-35 = 0.366-0.452	Æs. I. 33-34 = 0.231-0.366
----------------------------	----------------------------

## Current turned on :

—	+
Æs. I. 33-34 = 0.231-0.366	Æs. I. 32-33 = 0.192-0.231
Æs. I. 33-34 = 0.231-0.366	Æs. I. 34-35 = 0.366-0.452

## SUBJECT B, 23 May, 1910.

## Threshold before current was turned on :

Æs. I. 32-33 = 0.192-0.231	Æs. I. 29-30 = 0.148-0.154
Æs. I. 33-34 = 0.231-0.366	Æs. I. 29-30 = 0.148-0.154
Æs. I. 33-34 = 0.231-0.366	

## One cell turned on :

—	+
Æs. I. 33-34 = 0.231-0.366	Æs. I. 31-32 = 0.173-0.192

## Two cells turned on :

—	+
Æs. I. 32-33 = 0.192-0.231	Æs. I. 31-32 = 0.173-0.192

## Current reversed :

+	—
Æs. I. 34-35 = 0.366-0.452	Æs. I. 30-31 = 0.154-0.173
Æs. I. 33-34 = 0.231-0.366	Æs. I. 30-31 = 0.154-0.173
Æs. I. 34-35 = 0.366-0.452	Æs. I. 29-31 = 0.148-0.154

## Current broken :

Æs. I. 33-34 = 0.231-0.366	Æs. I. 31-32 = 0.173-0.192
----------------------------	----------------------------

## Two cells turned on :

—	+
Æs. I. 33-34 = 0.231-0.366	Æs. I. 31-32 = 0.173-0.192 <sup>2</sup>

## SUBJECT F, 1 April, 1910.

## Threshold before the current was turned on :

Æs. I. 36-37 = 0.606-0.980	Æs. I. 36-37 = 0.606-0.980
Æs. I. 37-38 = 0.980-1.539	Æs. I. 36-37 = 0.980-1.539

<sup>1</sup> Note the cathodic effect traveling to the anode. The next reversal, however, shows that a true anodic and cathodic effect is present.

<sup>2</sup> Here the experiment had to be interrupted. Had it been continued, anodic and cathodic effects probably would have appeared.

Threshold after current was turned on :

+	—
Beyond æs. I. 38 = 1.539	Æs. I. 35-36 = 0.452-0.606
Beyond æs. I. 38 = 1.539	Æs. I. 35-36 = 0.452-0.606

Current reversed :

—	+
Æs. I. 35-36 = 0.452-0.606	Beyond æs. I. 38 = 1.539
Æs. I. 35-36 = 0.452-0.606	Beyond æs. I. 38 = 1.539

Current reversed :

+	—
Æs. I. 37-38 = 0.980-1.539	Æs. I. 35-36 = 0.452-0.606

Current broken :

Æs. I. 37-38 = 0.980-1.539	Æs. I. 36-37 = 0.606-0.980
----------------------------	----------------------------

SUBJECT F, 24 May, 1910.

Threshold before the current was turned on :

Æs. I. 35-36 = 0.452-0.606	Æs. I. 36-37 = 0.606-0.980
----------------------------	----------------------------

Threshold after current was turned on :

—	+
Æs. I. 34-35 = 0.366-0.452	Æs. I. 32-33 = 0.192-0.231

Current reversed :

+	—
Æs. I. 37-38 = 0.980-1.539	Æs. I. 32-33 = 0.192-0.231
Æs. I. 37-38 = 0.980-1.539	Æs. I. 33-34 = 0.231-0.366

Current reversed :

—	+
Æs. I. 37-38 = 0.980-1.539	Æs. I. 34-35 = 0.366-0.452
Æs. I. 35-36 = 0.452-0.606	Æs. I. 34-35 = 0.366-0.452

Current reversed :

+	—
Æs. I. 37-38 = 0.980-1.539	Æs. I. 33-34 = 0.231-0.366
Æs. I. 37-38 = 0.980-1.539	Æs. I. 35-36 = 0.452-0.606
	Æs. I. 33-34 = 0.231-0.366

Current broken 3:17 p. m.

3:24 p.m. does not feel æs. I. 38 = 1.539	3:22 p.m. æs. I. 35-36 = 0.452-0.606
3:26 p.m. does not feel æs. I. 38 = 1.539	3:25 p.m. æs. I. 36-37 = 0.606-0.980
3:30 p.m. does not feel æs. I. 38 = 1.539	

In the following experiment I investigated with subject 'F.' (1) a spot in the neighborhood of the upper electrode which was the anode throughout, (2) a spot about 1 cm. below the upper electrode, and (3) a spot in the middle between the two electrodes. My object was to study the effect of a weak and strong current.

	Spot <sup>1</sup> at Anode.	Spot <sup>2</sup> 1 cm. Away from Anode.	Spot <sup>3</sup> Half-way Be- tween Anode and Cathode.
Threshold before current was turned on.	Æs. I. 35-36 = 0.452-0.606	Æs. I. 33-34 = 0.231-0.366	Æs. I. 35-36 = 0.452-0.606
Current on (two cells).	Æs. I. 34-35 = 0.366-0.452	Æs. I. 33-34 = 0.231-0.366 (3.00 P.M.) Æs. I. 31-32 = 0.173-0.192 (3.07½ P.M.)	Æs. I. 32-33 = 0.192-0.231

These results show that the cathode effect has traveled far over into the region of the anode.

	1	2	3
Current off 2 minutes 4 cells then turned on.	Æs. I. 35-36 = 0.452-0.606	Æs. I. 32-33 = 0.192-0.231 (3.14 P.M.) Æs. I. 33-34 = 0.231-0.366 (3.18 P.M.)	Æs. I. 35-36 = 0.452-0.606

From this table it appears that with double the strength of current spot (3) no longer suffers a reduction of its sensibility. Spot (2) with increase of time decreases in sensibility rather than increases as it did with the weak current. Spot (1), however, has returned to its normal threshold without being reduced in sensibility while the current was on.

	1	2	3
Current off at 3.24½ P.M.	3.38 P.M. æs. I. 35-36 = 0.452- 0.606	3.36½ P.M. æs. I. 31-32 = 0.173- 0.192 3.41 P.M. æs. I. 30-31 = 0.154- 0.173	3.34 P.M. æs. I. 34-35 = 0.366- 0.452 3.42 P.M. æs. I. 33-34 = 0.325- 0.366

Here we see that on breaking the current there was an increased sensibility at the anode judging by spot (2) and (3) which by analogy with the motor nerve was something to be expected. Spot (1) seems to have been not so amenable to the influence of the current. I have several times found similar cases and have explained them on the supposition that the direction of the current varies owing to irregularities of pressure or

moisture at various points of the bracelets. But if one will examine Pflüger's curve for the irritability of muscle one will see that a minimum effect is often to be found in the immediate region of the electrode.

A careful study of all these results will, I think, lead one to the conclusion that Pflüger's law for the irritability of motor nerves and muscles holds also for the skin. Sensibility as well as irritability is increased at the cathode and diminished at the anode.

#### VII. INFLUENCE OF THE INDUCED CURRENT ON THE SENSIBILITY OF THE SKIN.

When we treat an area of the skin with an alternating current applied by means of the electrodes (metallic) of an induction coil, two phenomena are to be observed immediately after removing the electrodes. (*a*) The sensibility of the skin is considerably lowered, (*b*) a spot which was before a touch spot has been transformed apparently into a pain spot.

I will give the laboratory notes for one experiment on this point, which can be verified repeatedly without any trouble. The induced current I used was always somewhat painful. Fräulein Suslowa found a reduction of sensibility even when the current was so weak that it could not be felt.

SUBJECT F, February 10, 1910.

Touch spot feels æs. III<sup>a</sup> at 5 = 0.692 gm. clearly.

Induction coil electrode applied.

Æs. III<sup>a</sup> 40 with hard pressure feels sharp.

Æs. IV. 27 = 22.8 gm. felt.

Electrode applied again :

Æs. IV. 27 = 22.8 gm. not felt.

When I had finished making the above entry

Æs. IV. 27 was felt.

Electrode applied again :

Æs. IV. 30 = 25.0 gm. felt like the prick of a needle.

When I had finished making entry

Æs. IV. 30 felt like a touch.

I then sought a pain spot. I found one with a threshold, Æs. IV. 15-20 = 3.401-6.280 gm. Touch was felt below 20 but at 20 it felt a little sharp.

Electrodes applied. Nothing felt at 20 Æs. IV. = 6.280 gm.

" " Touch felt at 25 " 11.7 "

" " Nothing felt at 25 " 11.7 "

Electrodes applied.	Sharp pain at	30	"	25.0	"
"	"	Nothing felt at	25	"	11.7
"	"	Sharp pain at	30	"	25.0

These results show that the threshold for pain is raised by the induced current and that, too, in proportion to the time in which it acted.

I then sought a touch spot in the neighborhood of the above which I marked as accurately as possible without the use of a lens.

Before the electrodes were applied—

	Touch felt but no pain at	Æs. IV. 30=25.0	gm.
Electrodes applied.	Pain at	Æs. IV. 30	=25.0 gm.
"	"	Nothing felt at	Æs. IV. 25=11.7 gm.
"	"	Pain felt at	Æs. IV. 30=25. gm.
"	"	Nothing felt at	Æs. IV. 25

From this statement it would seem that the touch spot is capable of transformation into a pain spot by means of the induced current. I thought at first that the reason for this might be that the pain spots in the neighborhood of the touch spot were rendered hypersensitive by the induced current. The result would be that a stimulation of a touch spot by a relatively strong pressure would involve the stimulation of the hypersensitive pain spots in the neighborhood. I therefore made the above test experiment and found that the threshold of the pain spots was also raised by the induced current. Consequently, if after treatment with an induced current a touch spot becomes sensitive to pain it must be because chemical changes have taken place in the touch spot that alter the character of the sensation to which it gives rise. If this is the case, one would expect that the character of a sensitive spot in the skin is determined not merely by anatomical features, but also by chemical compounds or enzymes of some kind that are present in or near the sensitive spots.

In confirmation of the results that the threshold of the pain spot is raised by the induced current, the following experiment may be quoted:

SUBJECT Fe., July 27, 1910.

Threshold of pain spot on inner side, middle, of forearm.

20-25 Æs. IV. = 6.2-11.7 gm.

At 20 Æs. IV. = 6.2 gm. Subject only once felt a sticking sensation.



At 25 Æs. IV. = 11.7 gm. Subject always felt a sticking sensation.

At 30 Æs. IV. = 25.0 gm. Subject felt a sharp sticking sensation.

Electrodes of induction coil applied :

At 25 Æs. IV. = 11.7 gm. Subject feels nothing. After making entry in note-book subject felt a slight stick at æs. IV., 25 = 11.7 gm.

Electrodes applied again :

At 27 Æs. IV. = 25 gm. Subject when touched the first time felt nothing. The second time he felt a touch which perhaps had a faint coloring of pain.

Electrodes applied again :

At 30 Æs. IV. = 25 gm. No pain felt.

35 Æs. IV. = beyond 25 gm. Sharp pain. About one minute later at 25 æs. IV. = 11.7 gm. Subject felt a sharp sticking sensation.

Electrodes applied again :

At 25 Æs. IV. Subject was not certain whether he had been touched or not.

30 Æs. IV. Subject felt a slight touch as if it were something blowing.

This experiment indicates very clearly that immediately after the application of an induced current the threshold of a pain spot is considerably raised.

It will be interesting to append to this account an analysis of Fräulein Suslowa's<sup>1</sup> experiments in this field.

In 1863 Fräulein Nadjeschda Suslowa undertook to see if the then recently discovered laws of electrophysiology applied also to the sensory nerves of the skin.

She applied the electrodes of an induction coil to the back of the hand. The primary current was constantly interrupted. The strength of the current from the secondary coil was not, however, sufficient to be felt. She then used a human hair head to draw across the skin between the electrodes. With the current on the hair could not be felt. When it was turned off it could be felt. It could also be felt when drawn across the skin outside the area between the electrodes whether the current was on or off. The results were the same even when the current was increased to the point where it could be felt. In explanation of this phenomenon Fräulein Suslowa writes, "The results obtained can perhaps be thus explained: The stroking of the hand with the hair could be felt before the electrical stimulation because the touch stimulus affected only

<sup>1</sup> 'Veränderungen der Hautgefühle unter dem Einflusse elektrischer Reizung.' *Zeitschrift für rationelle Medicin*, III., Reihe, XVII., 1863, pp. 155-160.

individual points of the sensitive skin. But where a relatively large sensitive area is stimulated electrically (the stimulation, indeed, exists but not consciously) the touch stimulus is distributed over this entire area and consequently is too weak to be felt at any individual point" (p. 156). Fräulein Suslowa also investigated the effect of the interrupted current on the spatial threshold. To do this she made use of a compass whose upper portion was of ivory and whose tips were copper, and could therefore be used as electrodes. She then approximated the points of the compass till they were just above the spatial threshold. This being done, she turned on the current. The two points no longer appeared double but single. This result was especially clear on the tip of the tongue. Further experiments showed that the wider apart the points of the compass are placed the stronger must be the current in order that they may appear single. In explanation the author writes, "these phenomena are easily explained from the standpoint of Weber's theory. The electrical current stimulates all the circles of sensation lying between the points of the compass. But according to Weber's theory, in order that a double sensation may take place some of these circles must remain unstimulated" (p. 157). In order to test her theory on this point, she stimulated the area between the points with a brush and found that then too the double sensation disappears. In less sensitive portions of the skin in order to make the double sensation disappear one need not stimulate with the brush the area just between the points but may also stimulate an area pretty far (in ziemlich grosser Entfernung) away — but in a direction perpendicular to a straight line between the points of the compasses. In order to exclude the objection that such stimulation acted as a mere distraction of the attention, she stimulated areas that were on another part of the body from that of the double sensation and found that in such cases the double sensation rarely disappeared but simply became less clear.

Fräulein Suslowa also investigated the effect of the constant current. The current was supplied by two Bunsen cells. As electrodes she used zinc plates of various shapes which were laid on the skin by means of two linen bands saturated with

sulphuric acid. In circuit were a rheostat and a commutator. She first investigated the effect of the current in the region of the positive and negative poles. She placed two zinc bracelets around the forearm and with the brush stimulated the skin now in the region of the positive electrode now at the cathode. "The results with every strength of current remained the same: lowering of sensibility at the anode and heightening of it at the cathode. In order to do away with the objection that possibly this result is due to stimulating with the brush areas which, as a matter of fact, have a different sensibility, one has only to reverse the current" (p. 159). Similar results were obtained with thermal sensations. The author was able with the direct current to duplicate the results obtained for double sensations with the induced current—but only on the tongue. She also found that the power of discriminating between two points was heightened at the negative pole and lowered at the positive. Here Professor Setchenow, under whose directions the experiments were made, entered the following note: "I must acknowledge that on repeating these experiments on my own arm I could obtain no definite results. But since I have in the matter no such rich experience as Miss Suslowa, the fact must be left to her responsibility" (pp. 159-160). In conclusion the author mentions a final fact: "I have discovered that when one dips the arm into any indifferent liquid (*e. g.*, water or oil) which is of the same temperature as the arm the fineness of discrimination between two points is considerably increased" (p. 160).

#### SUMMARY OF RESULTS.

##### *A. Influence of the Temperature of the Skin on the Spatial Threshold.*

1. The spatial threshold varies with the temperature of the skin, decreasing from the lower temperatures on up to a point not far from 36° C. Beyond this point it increases with the rising temperature of the skin.

2. The reason for this is to be sought for in the fact that the change in the spatial threshold simply expresses the variation in the intensity of the stimulus due to the temperature of the skin.

B. *Influence of the Pressure of the Points on the Spatial Threshold.*

3. It is in general true' that at various temperatures of the skin a weak pressure of the points gives a larger spatial threshold than a strong pressure.

C. *Influence of the Temperature of the Skin on the Touch Threshold.*

4. The minimum threshold of touch is found when the temperature of the skin is between  $35^{\circ}$  C. and  $38^{\circ}$  C. Above and below these points the sensibility of the skin decreases.

D. *Influence of the Direct Current on the Sensibility of the Skin.*

5. Pflüger's law for the irritability of motor nerves and muscles holds also for the skin, so that the sensibility of the skin is decreased at the anode and increased at the cathode.

6. When the current is too strong this result may be obscured by the creeping of the anodic effect over into the region of the cathode.

7. When it is too weak it may be obscured by the reverse process—the creeping of the cathodic effect over into the region of the anode.

E. *Influence of the Induced Current on the Sensibility of the Skin.*

8. *Immediately* after the treatment of an area of the skin with the induced current the sensibility is considerably lowered. This is true, not only for touch spots, but also for pain spots.

9. A touch spot is under these conditions transformed into a pain spot.

THEORETICAL INTERPRETATIONS.

1. *Sensibility as a Function of Dissociation.*

As stated in the introduction, I was led to investigate the relation between the temperature of the skin and the threshold of touch by the conjecture that it might bear some relation to the law that governs the velocity of chemical reactions at different temperatures. The threshold might depend upon the velo-

city of a chemical reaction in this way: Suppose that a change in pressure disturbs the conditions of equilibrium in the chemical reactions that are constantly taking place in the tissues. When this equilibrium is disturbed some reactions will go on faster or slower than before. The result will be the production of some definite compound or compounds concerned in the stimulation. There must be a minimum amount of this compound that corresponds to the threshold of sensation. If this is the case, then the amount produced will vary with the temperature and time of stimulation. Since the time of a momentary stimulus will average up as a constant, then for the lower degrees of temperature a greater pressure would be needed and therefore a greater disturbance of chemical equilibrium in order to produce a threshold stimulation. The investigation of this problem would be of great theoretical interest, but I do not as yet see the way to solve it on the basis of my present crude data. Furthermore, the curve showing the relation between the sensibility of the skin and its temperature has a maximum between 35 and 38° C. It is not likely that the velocity of reaction would show a maximum at this point. This difficulty might be explained by supposing that the stimulation is due to the formation of some compound which commences to break up or coagulate or precipitate at temperatures above that of the maximum of the curve obtained.

But there are other factors besides the velocity of reaction to be taken into consideration, and some one of them perhaps may be the dominant one in the variation due to temperature. Among these factors that of dissociation is of prime importance. The steps by which I have been led to this conclusion are as follows:

(a) The concentration of certain ions — especially Na and Ca, are known to be of great importance in the phenomenon of muscular irritability.<sup>1</sup> It might also be of importance in the sensibility of the skin. One way of varying the concentration of the ions in the tissues is by influencing, through changes in temperature, the degree of dissociation.

(b) As a general rule dissociation increases with rising temperature, but it has been shown by Arrhenius<sup>1</sup> that under certain

<sup>1</sup>Cf. Loeb, *The Dynamics of Living Matter*, Lecture V.

conditions there must be a maximum temperature beyond which the degree of dissociation decreases. This he showed to be actually the case, and consequently the sensibility of the skin might show a maximum at the point of maximum dissociation — if it were dependent on the concentration of some ion or ions in the tissues.

(c) The next step would be to actually measure the degree of dissociation in the blood and tissues at various temperatures. Since dissociation varies directly as the conductivity, this measurement is not difficult in animals like the frog and turtle, whose organs and muscles can function for some time after excision. This work has been done by G. Galeotti<sup>2</sup> for the frog and turtle. In living tissues he found an increase of conductivity from 12° on to a point at which he said the tissue commenced to die. The conductivity then decreased until coagulation, after which it rapidly increased with rising temperatures. The point of the first maximum varied with the tissue from 29°–35°. Dead tissues do not show this maximum and minimum, but only a constant increase along a parabolic curve.

The actual maxima for the different organs were :

The kidneys, about	32°.
Liver, between	32° and 34°.
Spleen, “	30° “ 32°.
Muscles, “	28° “ 30°.

Other interesting results were the following : After an organ has been removed from the body its conductivity decreases until a minimum is reached, after which it commences to increase. When decay sets in the conductivity reaches very high values. After the tissue has been killed by heat or cooling the conductivity decreases. The contractility of a muscle seems to be a function of its conductivity. The conductivity of the blood continues to increase with rising temperature even after coagulation.

The maximum and minimum values in the curve of conductivity for living tissues under the influence of heat, may be due, says Galeotti, to one of two causes —

<sup>1</sup> Ueber die Dissociationswärme und den Einfluss der Temperatur auf den Dissociationsgrad der Elektrolyte,' *Zeitschrift für physikalische Chemie*, 1889, IV., pp. 96–116.

<sup>2</sup> Ueber die elektrische Leitfähigkeit der tierischen Gewebe,' *Zeitschrift für Biologie*, 1902 (XLIII.), XXV., pp. 289–340.

The data obtained for the muscles were as follows :

Temperature. Degrees.	Cross Cut Frog Muscle. Living Tissue.	Longitudinally Cut Muscle of the Turtle. Living Tissue.	Gastrocnemius of the Frog. Living Tissue.	Muscles of the Turtle. Dead Tissue.
14	102.9	—	—	—
16	111.3	—	—	—
18	112.3	—	—	—
20	114.4	—	—	—
22	116.6	66.4	32.7	—
24	124.2	69.9	34.2	(72.2)
26	126.3	74.7	35.2	80.4
28	128.2	78.3	36.1	84.8
30	130.6	71.1	39.0	89.1
32	130.4	75.3	38.1	96.1
34	130.4	75.6	37.8	102.3
36	130.3	74.7	37.4	110.7
38	124.4	72.0	36.8	(123.0)
40	158.9	74.1	35.7	(134.8)
42	176.1	77.4	38.0	145.4
44	194.2	88.2	43.1	153.2
46	206.8	93.0	44.8	178.3
48	216.0	106.5	47.4	(193.0)
50	226.6	129.6	52.1	205.8
52	236.0	—	—	(219.0)
54	256.0	—	—	242.1
56	282.1	—	—	(269.0)
58	378.4	—	—	(306.2)
60	708.0	—	—	(355.0)
62	1010.0	—	—	409.5

(a) The internal friction of the protoplasm is increased after death by the coagulation of colloidal bodies. The conductivity of the colloids would thus be decreased and therefore the entire conductivity lowered.

Against this view he mentions the fact that experiments by Arrhenius,<sup>1</sup> Tietzen-Hennig,<sup>2</sup> Lüdeking<sup>3</sup> and Levi<sup>4</sup> show that coagulation does not introduce any marked changes in conductivity. The author himself, and also Sabbatini,<sup>5</sup> found that the conductivity of the serum is not changed after coagulation.

<sup>1</sup> Contributions to our Knowledge of the Action of Fluidity on the Conductivity of Electrolytes' (translation of Arrhenius' article by Professor Ramsey). Report of the British Association for the Advancement of Science (1886), 1887, pp. 344-348.

<sup>2</sup> 'Ueber scheinbar feste Electrolyte,' *Annalen der Physik. und Chemie*, 1888, XXXV., pp. 467-475.

<sup>3</sup> Lüdeking, Ch., 'Leitungsfähigkeit gelatinshaltiger Zinkvitriollösungen,' *Annalen der Physik und Chemie*, 1889, XXXVII., pp. 172-176.

<sup>4</sup> Levi (I have not seen this article), *Gazz. Chim. ital.*, 1900, XXV., ii., p. 64.

<sup>5</sup> Louis Sabbatini, 'Détermination du point de congélation des organes animaux,' *Journal de physiologie et de pathologie générale*, 1901, III., pp. 939-950.

(b) The second possibility is that the number of free ions in solution is decreased. As to the way in which this is done he suggests that as the protoplasm dries the ions are combined with the protein molecule and can therefore no longer assist in the conduction of the current. The life of the protoplasm would therefore be characterized by its higher ionization.

On this theory could be explained:

(a) The initial rise in the curve as due to the increased mobility of the ions brought about by the rise in temperature—their number remaining constant.

(b) The maximum at which the protoplasm commences to die and enter into combination with the ions. That this point is not sharp in the kidney and liver is due to the fact that these organs possess in their intercellular space very rich stores of ions.

(c) The fall in the curve is due to the continuation of the process of combination between the protoplasm and the ions.

(d) The subsequent rise of the conductivity curve which is close to the point of coagulation, might be due either to the fact that (a) the compound between the protoplasm and the ions is suddenly broken up, or that (b) the coagulation of the protoplasm breaks the cell walls and membranes of the tissues, thus allowing the ions to move with greater freedom.

If such salts as Na and Ca exist to any great extent as free compounds in the tissues of the skin and are not combined with complex proteid molecules in the living tissues, it is scarcely likely that the maxima found by Galeotti are due to conditions that determine the maximum in the curve of Arrhenius. Such salts increase in conductivity on up to very high temperatures. If the conductivity of the tissues has a maximum between 35 and 38° C. the main carrier of the current—to be subject to the law of Arrhenius—must have its maximum somewhere between those limits. This could only be the case if the salts of Na and Ca did not dissociate as independent compounds but because they were in some way taken up as constituent parts of a complex molecule in the living tissues.

Whatever may be the explanation of the phenomena the



fact remains that the degree of dissociation in various organs and tissues of the frog and turtle manifests a maximum in the neighborhood of  $30^{\circ}$  C. We have also found in the human skin a maximum sensibility between  $35$  and  $38^{\circ}$  C. Considering the fact that Galeotti was dealing with cold-blooded animals it is very likely that the maximum conductivity in the human tissues will fall within the limits of the minimum threshold. It is not, of course, logically demonstrated that because two things rise and fall together that therefore they are interdependent. But it constitutes very strong evidence that they are. Within the limits of the temperatures investigated by us one has but to apply a correction of temperature for the difference between warm and cold blooded tissues to see that sensibility follows the general course of the changes in the degree of dissociation. We may therefore lay down the law that *the tactual sensibility of the skin is a function of the degree of dissociation in the tissues.*

#### THE RELATION BETWEEN SENSIBILITY AND IRRITABILITY.

Not only is the sensibility of the skin at a maximum at the temperature of greatest dissociation, but also the irritability of a muscle. The experiments of Marey placed the maximum irritability of a muscle between  $30^{\circ}$  and  $35^{\circ}$  C. Gad and Heymans placed the maximum at  $30^{\circ}$  C. which is the maximum of conductivity in the above quoted tables of Galeotti's for the gastrocnemius of a frog.

Charles L. Edwards,<sup>1</sup> experimenting on the irritability of frogs' muscle and nerve, found a maximum for muscle between  $32.75^{\circ}$  and  $39.25^{\circ}$ . For curarized excised muscle he found maximum contractions at temperatures varying from  $33.25^{\circ}$  to  $38.25^{\circ}$ . For non-curarized excised muscle, at  $36.25^{\circ}$ ,  $29.25^{\circ}$ ,  $37.75^{\circ}$ , and  $38.25^{\circ}$ . The value  $29.25^{\circ}$  he looked upon as abnormal. He referred to Marey's<sup>2</sup> results, who obtained maximal contractions between  $30^{\circ}$  and  $35^{\circ}$ . Both investigators used

<sup>1</sup> 'The Influence of Warmth upon the Irritability of Frogs' Muscle and Nerve,' *Studies from the Biological Laboratory, Johns Hopkins University*, Baltimore, 1887-1890, IV., pp. 20, 35.

<sup>2</sup> E. J. Marey, *Du Mouvement dans les fonctions de la vie*, Paris, 1868, p. 353.

the same method — enclosure of the muscle in a double-walled cylinder through which water flowed. It is barely possible that Edwards did not allow time for the muscle to assume the temperature of the air in his cylinder, and thus thought it was at a higher temperature than it actually was. Marey's results are close to the maximum conductivity obtained by Galeotti.

Gad and Heymans<sup>1</sup> found that the frog's muscle gave its maximum at 30° both for isotonic and isometric contractions. For tetanic contractions they found the absolute maximum also at 30°, and a second relative maximum at 19°. The latent time they found to decrease constantly with rising temperature. The duration of the contraction also decreased with rising temperature and in accordance to a regular law, the curve being asymptotic to the axis of abscissas and perpendicular to that axis at 5°.

They were of the opinion that the two maxima they found pointed to the existence of two chemical processes involved in muscular contraction: *e. g.*, the formation of lactic acid and the breaking down of lactic acid into CO<sub>2</sub> and H<sub>2</sub>O.

One might suggest in passing that there are two phenomena in contraction, (*a*) that of irritability and (*b*) that of contractility. The irritability is a function of conductivity, having its maximum at 30°, as was found by Galeotti. The contractility is a function of the combustion of materials in the tissues dependent upon the amount present and the laws to which the chemical reactions involved in their combustion are subject.

Looking upon the results of Marey, Gad and Heymans as representing the true maximum of irritability (the temperatures given by Edwards being a little too high), we may lay down the law that *the maximum irritability of a muscle is at its temperature of highest dissociation*. This law is exactly analogous to the law of sensation. The two may be expressed as one by simply saying that *maximum sensibility for touch and the maximum of muscular irritability are both found at the temperature of greatest dissociation*.

<sup>1</sup> 'Ueber den Einfluss der Temperatur auf die Leistungsfähigkeit der Muskelsubstanz,' *Archiv für Anatomie und Physiol.* (Physiol. Abtheilung), Suppl. Band, 1890, pp. 59-115.

We have furthermore demonstrated that Pflüger's law for the effect of the constant current on muscles and nerves holds also—as has been hitherto suspected—for the sensibility of the skin; so that sensibility and irritability are both increased at the cathode and decreased at the anode. The decrease of sensibility at the anode might be due to an increase in the concentration of complex ions or, and this is more likely, because of a precipitate<sup>1</sup> found at the anode which is not easily disposed of by the circulation of the blood. At the cathode the increased sensibility is due to ions which migrate to the cathode and are of themselves sufficient to cause by their greater concentration a heightened sensibility, or can do so by entering into more complex, perhaps, proteid compounds.<sup>2</sup> The identity of the phenomenon in muscle, nerve, and sensory end organs teaches the important lesson that between these three tissues there is a fundamental relationship. This relationship consists in the fact that their diverse functions are dependent upon one and the same chemical phenomenon—the concentration of the ions in muscle, nerve and skin.

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<sup>1</sup> Cf. Oscar Gellner, 'Ueber Electrolyse thierischer Gewebe' (diss., Breslau, 1870, pp. 34).

<sup>2</sup> Cf. Loeb, *The Dynamics of Living Matter*, Lecture V. This lecture however refers entirely to the irritability of muscular and nervous tissue.

<sup>3</sup> This list is not meant for a complete bibliography. It is published in the hope that a list of the writers to whom I have referred may be of use to others.

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Two Points.	More than One.	Undecided.	One Point.
15.13	8.97	0.80	75.06

Besides four out of five subjects agreed that the Vexirversuch gave clear sensations of two points. Therefore the author is of the opinion that the delusion is in part due to subjective causes.

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Conclusions: Pain nerves are most superficial and are least sensible to heat. Next come the cold nerves. Deepest lie the heat nerves which are the most sensitive.

(For a criticism of Thunberg cf. Paul Bader, 'Das Verhältniss der Hautempfindungen und ihrer nervösen Organe zu calor. mech. und farad. Reizen.' *Philos. Stud.*, 1903, XVIII., pp. 437-477.)

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MAUREL, M. E. 'Considerations générales sur le zéro physiologique.' *Comptes Rendus de la Société de Biologie*, 1905 (LVII.), I., pp. 994-996. — The physiological zero is the temperature which in contact with the various parts of the organism gives neither a sensation of heat nor of cold. It varies from 29-32 in the normal conditions. In bed and especially in the evening it is about 33-34.

Cf. also *l. c.*, pp. 412-414 and 591-593.

Measured temp. by getting into a bath and noting at what temperature the water was perceived as hot or cold. Measured also temperature between clothing and skin.

OEHLER, J. 'Ueber die Hauttemperatur des gesunden Menschen.' *Deutsches Archiv für klinische Medizin*, 1904, LXXX., pp. 245-262. — Measured temp. by laying a thermometer on the skin. Temperature of skin dependent on that of room temperature varied from  $34^{\circ}$ - $35.05^{\circ}$ , while room temp. varied from  $20^{\circ}$ - $19^{\circ}$  to  $25^{\circ}$ - $24^{\circ}$ . Refers to results of Kunkel ('Ueber die Temperatur der mensch. Haut,' *Zeitschr. f. Biologie*, 1889) who obtained a skin temp. of  $32.5$  to  $34.4^{\circ}$  at a room temperature of  $20^{\circ}$ .

RENSSEN, W. 'Historisch-critisch en experimenteel onderzoek van de periphere temperatuur bij den gezonden mensch.' Diss., Groningen, 1884, pp. 86. — Mentions Sanctorius (1561-1636) as the first man who made use of a thermometer.

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VERESS, DR. ELEMÉR. 'Beiträge zur Kenntniss der Topographie der Wärme-Empfindlichkeit.' *Archiv für die ges. Physiologie* (Pflüger's), LXXXIX., 1902, pp. 1-86. — Measures degree at which skin in all parts of the body feels a sensation of warmth ( $39$ - $43^{\circ}$ ) and that at which the sensation of warmth passes into pain ( $40$ - $48^{\circ}$ ).

WITKOWSKI, ALBERT. 'Ueber Hauttemperaturen.' Diss. Berlin, 1883, pp. 30. — Quotes results of one J. Davy who measured the temperature of the skin by simply laying a thermometer on the skin and covering up the exposed side. His results varied in different parts of the body from  $32.26^{\circ}$  C. -  $35.80^{\circ}$  C. Witkowski's own experiments had to do with pathological conditions.

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$$\text{Conductivity} = \lambda_t = A_1 e^{-bt} (1 + at) \quad (\text{p. 112}).$$

— 'Contributions to our Knowledge of the Action of Fluidity on the Conductivity of Electrolytes' (a translation of an article by Arrhenius by Professor Ramsey) Report of the British Association for the Advancement of Science (1886), 1887, pp. 344-348.

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GRAEBER, ROBERT. 'Untersuchungen über den Einfluss galvanischer Ströme auf den Tastsinn der Haut.' Diss., Bonn, 1884, pp. 21.

KORÁNYI, A. V. 'Ueber die Reizbarkeit der Froschhaut gegen Licht und Wärme.' *Centralblatt für Physiologie*, 1892-1893, VI., pp. 6-8. — Reflex caused by light on skin.

KORÁNYI, A. V., and RICHTER, P. F. 'Physikalische Chemie und Medizin.' Leipzig, 1907.

LESSE, WERNER. 'Über den Einfluss hoher Temperaturen auf die Leistungsfähigkeit der Froschmuskeln.' Inaug. diss., Würzburg, 1893, pp. 39. — A criticism of the article by Gad and Heymans (*Archiv für Anat. und Physio., Physiol. Abth., Suppl.*, 1890, pp. 59-115). Maintains that to Gad and Heymans' law "Die Erschlaffungsprozess folgt um so schneller auf den Verkürzungsprozess, je höher die Temperatur" a second law must be added, viz. Bei hohen Temperaturen erleidet daneben der Muskel, ehe er wärmestar wird eine dauernde Schädigung seiner Erregbarkeit gegen elektrischen Reize.

He confirmed Dr. Emil Heubel's result with the heart for the muscle. As Heubel found that the heart is made active again by causing blood to flow through it, so Lesse had partial success in rejuvenating the frog's muscle in the same manner.

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## ON THE GENESIS AND DEVELOPMENT OF CONSCIOUS ATTITUDES (BEWUSTSEINSLAGEN).<sup>1</sup>

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Those interested in psychological research are familiar with the recent attempts at experimental investigation of the higher thought processes, and with the growing tendency toward the recognition of 'non-sensory' elements as essential components in conscious experience, which the results of these investigations have produced.

Stout (1896) was one of the first psychologists to maintain the occurrence in consciousness of imageless thought. "There is no absurdity," he says, "in supposing a mode of presentational consciousness which is not composed of visual, auditory, tactual and other experiences derived from, and in some degree resembling in quality, the sensations of the special senses; and there is no absurdity in supposing such modes of consciousness to possess a representative value or significance for thought."<sup>2</sup>

Mayer and Orth, in their qualitative study of association by the word method (1901), found that words were sometimes recalled by means of interpolated processes which might take the form of volitions, words, or 'peculiar conscious processes not characterizable in detail.'<sup>3</sup> Marbe, in his experimental study of judgment (1901), describes the same phenomenon and gives a long descriptive list of these attitudes, but declares that they cannot be adequately described. They were regularly characterized as 'peculiar, indefinite, or indescribable.'<sup>4</sup> Orth, in his *Gefühl und Bewusstseinslage* (1903), concludes that the one characteristic common to the various imageless conscious processes was the quality of obscurity and intangibility. In summing up Orth's conclusions Titchener says,

<sup>1</sup> *The Ms. for this paper was received Sept. 1, 1910 (Ed.).*

<sup>2</sup> *Analyt. Psych.*, I., 85 f.

<sup>3</sup> Titchener, *Experimental Psychology of Higher Thought Processes*, 100 f.

<sup>4</sup> Titchener, *op. cit.*, 101 f.

"When we try to name them, or seek to describe them, we are simply translating, substituting known for unknown; in actual experience, the attitudes are peculiar modifications of consciousness, which cannot be identified with sensation or idea or feeling."<sup>1</sup> Ach's observers (1905) reported that they frequently had a peculiar consciousness of what they had just before experienced. "It is as if," they say, "the whole experience were given at once, but without specific differentiation of the contents."<sup>2</sup> Bühler, as a result of an exhaustive study of the psychology of the thought processes (1907) concludes that, "there are thoughts without any the least demonstrable trace of any sort of imaginal groundwork."<sup>3</sup>

Much theoretical speculation has followed the announcement and description of these attitudes. Some have regarded them as forms of feeling, and have classed them along with Wundt's feelings of strain, relaxation, etc. Others have regarded them as a new conscious element, coördinate with sensation. A few have tried to account for them on purely physiological grounds. Others have been inclined to regard the whole contention between the 'imageless thought' advocates and the sensationalists, as a matter of individual difference in mental constitution. Some have argued that these attitudes probably represent the developed or automatized forms of imaginal processes made hazy and imageless by practice and use.<sup>4</sup>

The sensationalists have seemed most disturbed by the announcement and description of these imageless processes.<sup>5</sup> To them it has seemed as if all we have regarded as most reasonable and true in psychological interpretation was in danger of being overthrown, as if the possibility of a rational interpretation of psychic phenomena was being destroyed. They have argued that the development of the peripheral nervous system preceded the development of the central parts; that afferent

<sup>1</sup> Titchener, *op. cit.*, 102 f.

<sup>2</sup> Titchener, *op. cit.*, 103 f.

<sup>3</sup> Titchener, *op. cit.*, 144 f.

<sup>4</sup> Compare Titchener, *op. cit.*, 103-10, 180 f.

<sup>5</sup> Titchener, *op. cit.*, especially lecture V. Washburn, 'The Physiological Basis of Relational Processes,' *Psych. Bull.*, VI., 1909, 369 f. Angell, 'Thought and Imagery,' *Philos. Rev.*, VI., 1897, 648 f. *Ibid.*, VII., 1898, 74 f.

changes produced in the nervous system were the first kind of neural processes that occurred, and therefore (phylogenetically speaking) the source of all consciousness; that the process initiated in a sensory nerve ending, is, so far as we know, the one necessary antecedent condition of a change in consciousness; that these attitudes were so nearly sensory in nature that better observation and a more careful analysis would, doubtless, prove them wholly so.

The investigators, on the other hand, have been just as persistent in proclaiming that these 'non-sensory' processes exist. They have been steadily describing new phases and aspects of this group of phenomena. Most psychologists have become convinced not only that attitudes exist, but that they represent conscious processes which are in reality imageless.<sup>1</sup> All have been trying to account for these attitudes, but so far as the writer is aware no one has been able to give a satisfactory explanation (one based on controlled observation and experiment) either of their origin or nature and cause.

It is the purpose of this paper to present certain facts revealed by an experiment which the writer made some years ago, that throw important light on both these points — facts not published hitherto which reveal the nature and origin of several of these attitudes. The writer has no theory to advocate or defend and merely wishes to present the facts irrespective of any wider psychological implications which they may have.

#### SOURCE OF DATA AND EXPERIMENT.

The facts to be described were obtained from an experiment in learning typewriting, which the writer began in the psychological laboratory of Clark University, some five years ago, and published as Volume I. of the *University of Montana Studies in Psychology*, December, 1908. As may be inferred, the facts here to be presented fell outside the problem set in that experiment. They have not been written up before because of a pressure of teaching duties which prevented the writer from correlating them with the results of recent investigations of the same phenomena. It was the publication of Titchener's

<sup>1</sup> Washburn, *op. cit.*, 369.

book on *Experimental Psychology of the Higher Thought Processes* which brought to the writer's mind afresh the value and significance of this group of facts.

In the experiment referred to, a number of subjects,<sup>1</sup> including the writer (subject *X*), were given the task of learning to use a typewriter, without any directions as to methods of learning. They learned to write by both the sight method, in which they were privileged to look at the keyboard, and the touch method, when the whole keyboard was carefully screened from their view. A definite amount of practice was taken, at a stated time, each day. The main problem of the research was to determine accurately the rate of progress in learning, and to obtain from each of the learners a complete objective and psychological record of the learning.

For obtaining an introspective analysis of the learning consciousness for all levels of advancement, a plan was devised by means of which a careful cross-section analysis of the learning consciousness could be obtained from each of the learners, for all levels of advancement. In these cross-section analyses the attempt was made to carefully and systematically observe and describe all conscious processes which preceded or accompanied the making of the writing movements, or which were in any way involved in successfully carrying on the work. By making such cross-section analyses at all levels of advancement in the learning, a more or less complete psychological history of the entire process of learning was obtained. Each conscious process involved in the writing was observed, not merely at a given stage of its development, but at every stage, and the important changes in its formation and development noted and described.

This *genetic method* of observation enabled us to determine not only the specific associations and habits that were acquired in the course of the learning, it revealed exactly how they were formed and perfected. With the formation and development of the various habits acquired in the course of the learning, we are not here concerned. It was the facts revealed by the care-

<sup>1</sup> Compare Book, 'The Psychology of Skill,' *Univ. Mont. Pub. in Psy.*, I., 12 f.

ful descriptions, which the learners made of the conscious processes that preceded and accompanied the execution of the writing movement in the different stages of advancement, that proved important for explaining the nature and origin of the attitudes operative in the more expert stages of the writing.

But before describing these facts the general method used for obtaining and controlling these observations should be further explained. Two kinds of introspective data were gathered in our experiment. The first consisted of the observations made by the learners during the course of the regular daily tests, taken to determine their rate of progress. It was a part of the regular program to have each learner describe immediately after finishing a test such parts of his whole conscious experience as he could recall. After this he was usually questioned by the experimenter about how the work had been done on that day, and about the things which occurred to help or retard his progress. This group of data proved especially valuable for disclosing some of the factors and conditions which helped or hindered the acquisition of the special habits formed, and is irrelevant to our present purpose. The facts in these daily records which described the regular conscious processes involved in the writing were tabulated and correlated with observations made in other special practice periods arranged for making the cross-section analyses mentioned above.

These cross-section analyses were made as follows: At irregular intervals throughout the practice the learners were set to writing (copying, all writing was from copy) for the purpose of determining just how the writing at that stage was done, *i. e.*, for observing the various conscious processes which *regularly* accompanied or preceded the making of the writing movements. (The fluctuations or variations in consciousness, due to chance associations, distractions, etc., were purposely ignored in these special practices.) These special periods of practice were taken in the early stages of the learning, when changes occurred rapidly, every day, later every second, third, fifth or tenth day, as the rate of progress demanded, and were continued in each case until a complete cross-section analysis of the learning consciousness, for that stage of advancement, had

been obtained. During these special practice periods the subjects always wrote at their maximum rate, but were *privileged to stop at any time for the purpose of making* and recording observations. They were also told to stop to rest whenever they began to feel fatigued.

During these special practice periods the observers were alone in the room and made their observations unaided by the experimenter, except that they were provided with an outline giving specific directions for making an accurate and complete cross-section analysis of the learning consciousness.

The following special precautions were observed in making these cross-section analyses. As every one knows, making a thorough analysis of so complex a state of consciousness as is involved in trying to use a typewriter, is by no means an easy task. It is like looking at a forest. You can't see it all. You can observe only a small part of what is going on. Part of what you observe gets away before you can write it down. Much of what transpires cannot be adequately, or perhaps correctly expressed. There is special need, therefore, that the observations be repeated until important facts can be determined and verified.

The nature of our experiment proved especially valuable in this regard. In using a typewriter certain definite things must be done. The keys to be struck must be located (mentally and with the fingers), the several letter-making movements must be initiated and properly guided, the words to be copied must somehow be held in mind until written with the machine, etc. The special problem of the observer being to ascertain how these several tasks were performed at the level of advancement where the cross-section analysis was being made, he could take up these problems one at a time, and make repeated observations on that part of the conscious field until the facts pertaining to the solution of a problem had been clearly determined and verified. It was the regular plan to analyze the learning consciousness in this way, passing from one part or problem to another until the whole learning consciousness had been analyzed. The changes, due to practice, were not so rapid but that they remained practically the same during one of these

special practice periods. The continued writing, therefore, gave abundant opportunity for repeated observation of any conscious process involved in the writing.

Special care was also taken to guard against attaching undue importance to the various facts or phenomena observed, and to continue each special practice period until all important facts had been determined and verified. Besides the direct control which the observers had over this difficulty, objective records of the writing were obtained in all the tests, which pictured concretely on a revolving drum all that the subjects did at the machine.<sup>1</sup> These records were correlated with the special introspective notes and helped to check, verify and direct the observations of the learners.

The utmost care was also taken to have the observers describe only what occurred in consciousness, without any alterations or additions. The danger from this source of error is of course very great, but was guarded against by selecting experienced observers and having them keep clearly in mind the need of making a careful distinction between psychological processes and facts, and a logical report of the same. If this is kept in mind and the observations are repeated as stated above, the most complex conscious states may be accurately analyzed.

### RESULTS.

A careful examination of the data obtained from these several cross-section analyses of the learning consciousness showed that the 'mental adjustments,' 'sets of mind,' 'determining tendencies,' or conscious attitudes which were operative in the more advanced stages of the practice, represented, and, in fact, were nothing more nor less than the developed forms of the representative processes or images, operative as directing forces in the early stages of the writing. It was clearly determined that all the images which appeared in the different stages of the writing to initiate, guide, or direct the movements, were first prominent and distinct, then hazy or vague, giving way, finally, to the mental adjustments, sets of mind, determining tendencies or conscious attitudes which later initiated and con-

<sup>1</sup> Compare Book, *op. cit.*, 9 f.

trolled the writing movements.<sup>1</sup> The conscious attitudes developed by our subjects in learning to use a typewriter, therefore, represent the developed or automatized forms of certain definite representative processes, made imageless through much practice or use.

A concrete illustration will make this fact definite and clear.

In the earliest stages of touch writing the location of the letters on the keyboard was a very difficult task. Careful analysis showed that after the copy had been learned so that a word or phrase could be held in mind until written, four distinct steps were required to make each letter. There was (1) an actual spelling or thinking of the letter, (2) a process of mentally locating this letter on the keyboard, (3) the finding of this letter with the fingers, (4) the initiation and making of the final letter-making movement.

Our cross-section analyses of the learning consciousness showed that in the earliest writing of all our subjects, each of these steps was initiated and controlled by vivid imagery. Definite representative processes were required to initiate and direct each step in the work. An actual spelling or pronunciation of the letter started the process in every case—a motorization so strong in some of our subjects that it could be distinctly heard by the experimenter who sat at a table ten feet away, operating the apparatus used to take a drum record of the writing. This spelling or motorization of the letter called up in the case of observer *X* a distinct visual image of the exact position of that letter on the keyboard. I quote from the notes: "I have to stop," he writes, "for each letter to recall by visual image where the key wanted is, then make the proper finger movements in the right direction and distance. In making each letter a distinct visual image of the letter in its exact position on the keyboard comes up to direct my fingers and hand." The process of finding a key with the fingers, when thus mentally located, was in the beginning a most laborious and difficult task, accomplished by the fingers literally feeling their way, step by step, to the position of each letter on the key-

<sup>1</sup> Compare Book, 'Psychology of Skill,' *op. cit.*, 42 note.



board. When the fingers had thus found the key to be struck a second spelling or motorization of the letter was required to set off the final letter-making movement.

The procedure of observer *X* was identical with the above, except that he used a different type of imagery to locate the letters on the keyboard, due to his method of learning the keyboard.<sup>1</sup> In his case the spelling or pronunciation of the separate letters at once called up a conscious process, regularly described as a movement of attention to the exact position of that letter on the keyboard. Careful analysis shows that this movement of attention, or idea of the position of the letter on the keyboard, was, in the earlier stages of the writing, nothing more nor less than the recall of the motor experiences — the eye, neck, and throat movements — involved in locating the keys on the visual map. I quote from his notes: "In taking up the problem of determining how I locate the letters on the keyboard again, I find that there is present, for each letter, what might be called a movement of attention to the exact position of the letters on the keyboard, following the spelling and preceding or going with the process of locating that key with the fingers. In analyzing this conscious process (movement of attention) more closely, I find that it is for the most part made up of actual and incipient eye movements in the direction of the keys, slight innervations felt in the regions of the eyes and throat, that can be clearly detected by placing the finger tips upon the closed eyes. I did not catch this process in its earliest stages of formation, but infer that this movement of attention represents the remnants of the actual eye movements involved in locating the keys with the eyes on the keyboard map, or in locating the letters on the keyboard with the eyes, in my former sight method writing."

It was this motor image or movement of attention that enabled observer *X* to locate the letters on the keyboard and start his fingers and hand in the right direction. When the fingers, thus directed, had felt their way to the proper key a second actual pronunciation of the letter was required to set off the movement for making it on the machine, as was the case with *Y*.

<sup>1</sup> Compare Book, 'Psychology of Skill,' *op. cit.*, 25 f.

Practice and increase of skill brought some very important changes in the conscious processes, which thus initiated and controlled the writing movements. By an elaborate process of short-circuiting, which need not here be described, direct letter associations were formed for each letter of the alphabet. These made it possible for the above-named steps to be taken as one. When letter associations became operative, getting the copy was an exceedingly easy task. The eyes were kept focused on the printed page, and the copy was gotten as written, letter by letter, and as a pure visual sensation. I quote from the notes: "The mere sight of the letters in the copy, at once sets off the movements required for making them. Visual recognition is all that is needed to initiate the proper letter-making movement." No initial or second spelling was required. The visual fixation of a letter in the copy at once called up the idea of its exact position on the keyboard, a process which came to be followed so closely, in this stage of advancement, by the hand and finger movements required for making it, that the whole process could be controlled by a single conscious span.

As these letter associations were developed and perfected an important change took place in the conscious processes involved in locating the letters on the keyboard. The notes show that the imaginal processes which formerly gave direction to the fingers and hands became, as letter associations were perfected, more and more incipient and harder and harder to observe and describe. The idea of the location of the letter on the keyboard became so imageless that the learners regularly declared it could not be described long before it disappeared from consciousness. The notes clearly show that the fingers and hands came to be guided, as to direction, by some sort of 'set of mind' which the learners felt wholly unable to analyze and describe. The notes further show that this 'mental attitude' represented the developed form of the vivid representative process used to locate the keys in the earlier stages. To illustrate: The visual image used by observer Z' to locate the keys in the earliest writing was soon superseded by a *visual-motor* image 'which told him more quickly and surely where the key wanted was.' This in turn gave way to a process regularly character-

ized as 'a visual direction,' an imaginal process which became more and more incipient and formless until it became a mere 'set of mind' or 'mental adjustment' still necessary to give direction to the fingers and hands, but which *I* failed to further analyze and descibe.

The 'movement of attention' used by observer *X* to locate the letters on the keybord was in the earliest writing nothing more than the recall of the motor experiences involved in the location of the letters on the keyboard map, as we have seen. This imaginal process soon gave way to a movement of attention much more incipient, but still rich in motor imagery. This was in turn superseded by a sort of 'flow of attention' in which only vague traces of motor imagery could be detected. This gave way to a 'mental adjustment' or 'attentive attitude' so incipient and imageless that it could not be adequately analyzed or described. Long before the idea of the location of the letters on the keyboard disappeared from consciousness *X* repeatedly wrote in his notes, "The process must be introspectively experienced to be understood; it cannot be adequately described."

The process of locating the keys with the fingers underwent changes in this and in the beginning of the word association stage equally interesting and important. When the four steps required to make a letter in the preceding stage had been fused into one, so that the pronunciation or visual focusing of a letter in the copy at once called up the necessary hand and finger movement for writing it, the letter-making movements could be directed and controlled as made. The fingers were guided by keeping attention carefully focused on the 'feel' (motor-tactual image) of the movement. I quote from the notes: "By far the major portion of attention goes, in this stage, to the feeling of touch and movement. These are more or less constantly watched with respect to rightness and wrongness. I find that I can avoid errors much better by attending strictly to the 'feel' of the fingers and keeping constantly in mind where my fingers are. As soon as I fail to do this my fingers go in the general direction only, and miss." In other words, the fingers were directed, in this stage, by a motor-tactual image of the letter-

making movement. In the beginning this imaginal process was very definite and distinct. The letter-making movement had to be made very slowly, and carefully attended to throughout its whole course. Every turning point in the movement had to be zealously guarded by giving close attention to its motor-tactual 'feel.' I quote from the notes: "What occurs in making a movement slowly is an attentive following of the 'feel' of the movement throughout its whole course, a vivid imaginal process which is made as fully and concrete as the time will permit." But as the practice continued this motor-tactual image became less vivid and distinct. The letter-making movement came to be directed in a more general way. The vivid motor-tactual image slowly gave way to a 'half-conscious following of the movement,' as a whole, and this to a sort of 'pre-mental adjustment for that particular movement,' a 'directing tendency' felt to be present for weeks after the learners were unable to say anything definite about it.<sup>1</sup>

But before this conscious process became wholly devoid of imagery for every letter a new method of locating the keys, by the fingers, became operative. As syllable and word associations began to develop, attention, which was formerly attached to individual movements for purposes of guidance, became directed to the motor-tactual image of the finger movements involved in writing syllables and words taken as a group. I quote from the notes: "A word simply means a group of movements which I attend to as a whole. I seem to get beforehand a sort of 'feel' of the whole group, which is run through with that sort of conception and direction of attention." The notes not only show that such group images were developed for the syllables and words, but that in every case the motor-tactual image for such a group of movements was at first clear and distinct, later more general and indistinct, becoming finally a mere conscious attitude which controlled the group as a whole. I quote from the notes: "There is a change in attitude, or what not, towards the keyboard that goes with the increased ability of handling it. I have noticed it for several days, but hardly know how to describe it. It is something like getting a motor-

<sup>1</sup> Compare Book, *op. cit.*, 33, 40.

tactual image for the whole key-board. As I write along easily and pretty fast — this is when I am most aware of this process — I seem to feel beforehand in what direction the following movements are to be made, and seem further to have a vague motor knowledge of the positions of all the keys." Later the conscious process which controlled these groups of finger movements was regularly referred to as 'getting a right orientation' or 'mental adjustment' for that group of movements, *i. e.*, it became imageless. In the matter of the conscious processes which gave direction and control to the fingers and hands, we, therefore, have the development not merely of an attitude, but of a hierarchy of attitudes.

The same is true for the mental spelling. When the syllable or word first became the unit for attention, an actual mental spelling was required to initiate and control the sequence of the movements. This mental spelling was at first characterized as an actual motorization of each letter in the word. But as practice continued and the movements got themselves linked more closely together, in the writing of words, the mental spelling became more and more incipient, persisting as an imageless conscious process long after it ceased to be consciously attended to. I quote from the notes: "Just for an instant today, when my attention was half-directed to it, I found myself incipiently pronouncing every letter as I wrote. I had felt for a long time that no more spelling was done, but find that a kind of *mental adjustment* for each letter is needed to make the movements go right." The minute character of this 'mental adjustment' was not described in the notes, but the accounts of its growing incipency, and the many positive assertions, that such a set of mind was required to control the movements for so long a time after the spelling had seemingly disappeared, show that the former actual spelling had given way to a conscious attitude. The fact that this mental spelling slowly became linked with the conscious processes involved in locating the keys with the fingers, and that it was, in the course of the practice, superseded by a kind of group spelling, made it all the more difficult to observe and describe.

As word and phrase associations became perfected a group

spelling took the place of the former letter by letter method of spelling, above described. The letter-making movements soon became so closely linked in the repeated writing of certain phrases and words, and could soon be made so fast, that a conscious following of the individual movements became impossible, making it necessary that a higher method of controlling the sequence of the movements should be developed. The conscious processes involved in this 'group spelling' differed greatly, as to character and distinctness, with the stages of advancement. It was first characterized as 'a hurried anticipatory following of the whole course of the movements involved in writing the word or phrase,' a motor process rich in accents. This, in time, gave way to a characteristic pronunciation of the words. In describing how a certain word of two parts was written observer Z wrote: "I notice that I no longer spell it letter by letter, but that there are two distinct motor innervations necessary for writing the word, taking place before the first letter of each of these parts of the word is struck." As the practice continued this process of motor control became more and more incipient until it was regularly spoken of as 'getting the right mental adjustment' for a particular group of letters or words. The notes clearly show that an attitude of a higher order was developed. Here, as in the matter of locating the keys, a hierarchy of attitudes was developed. To what extent the conscious process which controlled the sequence of the movements partook of the nature of an attitude is suggested by the fact that our subjects could write a practice sentence correctly and at a maximum rate, while singing (*i. e.*, holding) the notes of a familiar tune.

A fact which gives strong support to the above statement, that certain attitudes or imageless conscious processes developed from definite representative processes, is the fact that none of these conscious attitudes was operative in the earlier stages of their development except on the good days and during the best periods of work.

In case of fatigue or when for any reason there was a slump in the learner's general physiological tone there was a regular return to an earlier level of work where the higher mode of controlling the writing gave way to a lower, the attitudes to the

more detailed representative processes which directed the writing in an earlier stage. No fact was so often described by all the observers as this slipping back from a level of control where the work was directed by conscious attitudes to a stage where a more detailed representative process was required, temporarily, to control the work. We, therefore, have in this group of facts data which proves the converse of the above proposition.

Similar illustrations might be drawn from the observations made of learning to write by the sight method.<sup>1</sup> Here, as in learning by the touch method, several attitudes were developed in the course of the practice. In the matter of spelling or initiating and controlling the sequence of the letter-making movements, in the location of the keys with the eyes, in the matter of developing a motor-tactual image, which came to assist the eyes in locating the keys, conscious attitudes were formed from the specific representative processes used in the earlier stages of the writing. As the practice continued conscious processes became operative that were as imageless and difficult to analyze and describe as any *Bewusstseinslagen* or 'imageless thought' processes which Messer, Bühler, Marbe, Ach, *et al.*, have described.

#### CONCLUSIONS.

If, as there seems every reason to believe, the conscious attitudes developed by our subjects in learning to use a typewriter were of the same general nature as the attitudes, which these investigators observed and described, we may fairly conclude:

1. That the attitudes they described, like the imageless processes developed in the course of our experiments, represent nothing more nor less than the developed forms of representative processes made imageless by practice and use. Ach, in his study of *Volition and Thought*, found that awarenesses (attitudes) of 'meaning' graded off into awarenesses (attitudes) of 'relation' through intermediate forms.<sup>2</sup> Woodworth in his rule-of-three method of studying thought processes found that, in supplying the missing terms in his syllogisms, the transfer of

<sup>1</sup> See Book, *op. cit.*, 54 f.

<sup>2</sup> Titchener, *op. cit.*, 107.

relation from the first pair of terms to the case suggested by the third was sometimes made as the result of the recall of a definite image or word; sometimes as the result of an imageless thought process; sometimes without any consciousness at all, simply as the result of the *Aufgabe*.<sup>1</sup> Messer, in his *Experimental Investigation of the Psychology of Thought*, determined the various stages of development or elaboration through which a thought process might pass in consciousness, and found that his attitudes went through several distinct transitional forms.<sup>2</sup> C. L. Taylor<sup>3</sup> notes that both the imaginal representation of meaning and the attitude of 'understanding' tend to lapse as a printed text becomes familiar (241, 246). Also that an observer, who finds visual ideas essential (229) or at any rate useful (235) in the solution of a given problem, drops these ideas and employs simply 'thoughts' and attitudes in the solution of further problems of the same kind (236).<sup>4</sup>

These facts taken with the results of our experiments seem to the writer to warrant the inference which the above statement implies.

2. We also conclude that Stout, Woodworth, Marbe, Bühler and Orth were right in asserting the existence of conscious processes which cannot be adequately described in representative terms. That Messer and Ach were also right in maintaining that their attitudes went through a process of elaboration or development which took them through several transitional forms. Finally that Angell and Titchener were also right when they said that the attitudes they observed were not in reality imageless. We infer that all observed the same group of phenomena, but at different stages of its development. Orth, Marbe, Woodworth and Bühler caught the attitudes at the high tide of their development. Titchener and Angell caught them before they had lost all traces of their former imagery. Messer and Ach observed the same conscious processes (attitudes) at different stages of their development. There is also abundant

<sup>1</sup> Titchener, *op. cit.*, 95 f., 152 f.

<sup>2</sup> Titchener, *op. cit.*, 112.

<sup>3</sup> 'Ueber das Verstehen von Worten und Sätzen,' *Zeits. f. Psych.*, XL., 1905. 225 ff.

<sup>4</sup> Titchener, *op. cit.*, 247.



evidence that some of the observers who took part in these experiments caught some of the attitudes when they had dropped almost below the threshold of consciousness.

Our results suggest that the attitudes stand mid-way between the vivid imaginal processes regularly operative in consciousness and such internal stimuli as auto-suggestions. That these subconscious stimuli occupy a place midway between the attitudes and instinctive stimuli. Conscious attitudes seem to represent a stage in a process of development which begins with vivid imaginal thought, and slowly and gradually passes downward to a stage of automatic or instinctive control.

3. Our results also show that methods for the study of psychological phenomena must be greatly refined and improved. We must not only improve our methods for making cross-section analyses of conscious processes and states, but apply, as Titchener has recently urged, the *genetic method* of observation to the study of all psychological phenomena. We have been studying conscious processes too much as if mental facts were mere static and unchangeable things. We have been so busy analyzing the tangles of special conscious states that we have failed to see important facts before our nose. We have forgotten that consciousness is a choice of processes each modified not only by what comes before and after it but that each of these processes goes through a more or less definite course of growth or development as the stream of experience widens and moves on. Every mental process should be viewed not merely as seen in cross-section analysis, it should be studied from the genetic point of view. Viewed in the light of what it was and is to be.

No one knows how many psychological caterpillars we have been calling worms; how many future mosquitoes we have been calling wiggle-tails because of our failure to do this. For ten years psychologists have been puzzled by the phenomenon represented by conscious attitudes. Many theoretical explanations have been given. The application of a carefully refined genetic method of observation to the part problems pertaining to these attitudes will tell us what they are. It is impossible to conjecture what the application of this method to the study of perception, memory, association and the higher

thought processes may not bring forth. The writer believes it will show that the abstract thoughts of the philosopher, and the psychology of faith and auto-suggestion, and belief are not such enigmas after all.

## REACTIONS TO RHYTHMIC STIMULI, WITH ATTEMPT TO SYNCHRONIZE.<sup>1</sup>

BY KNIGHT DUNLAP.

In my paper on 'The Complication Experiment and Related Phenomena'<sup>2</sup> I stated my conclusion that the typical illusion of the so-called 'complication experiment' depends on a rhythmic reaction which the subject makes mechanically: that the result of this reaction (the tolerably clear vision of the pointer) seems to the subject to be synchronous with the sound or whatever discrete stimulus is used, but is really achieved previous to the said stimulus (giving negative error), or subsequent thereto (giving positive error), in all cases in which the typical error is found.

Further work, with definite sorts of rhythmic reactions seems desirable, as I have already stated,<sup>3</sup> and the present paper is a report of a beginning in this work.

The phenomena of the 'complication experiment' depend on an indirect attempt to synchronize reaction with stimulus. Indirect, because the subject is little, if at all, conscious through muscular or tactual sense of the reaction, but apprehends the result, *i. e.*, the visual 'picking out' of the moving pointer and attempts to make this result synchronize with the rhythmically repeated stimulus. It would be possible to arrange an experiment with a definite rhythmic reaction (*e. g.*, a finger movement) which should be controlled by the consciousness of its visual or auditory results; but it has seemed advisable to work first with attempts at direct synchronization (that is, of the stimulus with the tactual and muscular content of the reaction consciousness). So far as I can see now, either method is equally good; for the difference between what I here designate as 'direct' and 'indirect' is after all a matter of degree. Actual comparison of the two methods will however be made later.

<sup>1</sup> From the Psychological Laboratory of The Johns Hopkins University.

<sup>2</sup> PSYCHOLOGICAL REVIEW, XVII., 157-191.

<sup>3</sup> *Op. cit.*, 191.

There seems to have been very little work done upon the specific problem of the synchronization of a definite phase of a rhythmically repeated reaction with the stimulus. I have referred in another place to the remark of Scripture.<sup>1</sup> Stevens,<sup>2</sup> in his records of rhythmic finger movements made no measurements of the reactions except for the period after the stimulus was suspended. Stetson,<sup>3</sup> basing his statement on combined reactions of finger and foot, and on the stroke on a piano key, says that "a sound occurring during the beat stroke is referred to the end of the beat stroke, and becomes a part of the limiting sensation." Obviously, the conditions in the reactions mentioned are not good for the decision of the question which most interests us: How near can a reactor come to the synchronization of a definite phase of the reaction with the stimulus?

Stetson's remark, although applied to beat strokes only, suggests the possibility that in synchronizing reactions in general, the particular phase of the reaction selected for synchronization with the stimulus is of critical importance. It might make a difference whether the beginning, the end, or some intermediate phase of the movement is chosen. On this point I have made experiments on all the reactors listed below, comprising about two thousand reactions, and similar experiments on other reactors, and have found that the phase of the reaction selected makes very little, if any difference in the error, and none whatever in the direction thereof. I have also compared the results with keys of the press and release type, with similar conclusion.

Planning to use finger movement, I had a key constructed which was designed to operate with a minimal amount of noise. A lever, 7 inches in length, with pivot in the middle, bears at one end a finger button, and at the other a steel point dipping into a mercury-cup. A coil spring of adjustable tension attached midway between pivot and point draws the point down, and a

<sup>1</sup>Dunlap, *op. cit.*, 177. Scripture, E. W., 'Observations on Rhythmic Action,' 1899, *Yale Psychological Studies*, VIII., 103.

<sup>2</sup>Stevens, L. T., 'On the Time Sense,' 1886, *Mind*, O. S., XI., 391-404.

<sup>3</sup>Stetson, R. H., 'A New Theory of Rhythm and Discrete Succession, 1905, *PSYCHOL. REV.*, XII., 293-350.

piece of catgut attached midway between pivot and finger-button stops the downward movement of the steel point at any desired position. There is no stop to the downward movement of the finger button. The key is mounted with a heavy metal base, so that it is not necessary to clamp it to the table. The mercury-level, spring, and catgut are finely adjustable, and the pivot is of the adjustable cone type, reducing friction to a minimum.

The key was so set that a very slight pressure on the button broke the contact between mercury and steel point. The subject was therefore instructed to make the touch of the finger-tip on the button synchronize with the stimulus.

The key, as operated, produced noise in two ways. First, from the impact of the finger on the button, which made a sound so slight as to be negligible. Second, from the checking of the lever by the catgut, when released, which was noticeable, but came so long after the chief phase of the reaction that it is hardly probable that it was a disturbing factor. An absolutely noiseless key with a satisfactory action, I have not yet succeeding in securing.

The reaction key was connected in series with the primary of an induction coil rated at three-quarter inch spark. A one microfarad condenser was connected in parallel with the break of the key. The primary current was six amperes at twelve volts. By this arrangement the break of the current, and therefore almost exactly the impact of the finger on the button, was registered by the spark from the coil, as will be further described below.

The stimulus, to which the subject reacted, was either the snap of an electric spark, passing between terminals supported about eight inches from the subject's right ear, or else the illumination of a sheet of white drawing paper by the flash of a helium tube. The sheet, fifteen by twenty inches, was supported vertically at a distance of approximately forty inches from the subject's eyes, and the helium tube was fifteen inches from and on a level with the top of the sheet. The tube was screened from the subject's eyes, so that he saw only the reflected light. By means of an ordinary double-throw double-

pole switch the spark-gap or the helium tube could be connected in the stimulus circuit at will.

The stimulus current was furnished by the secondary of a large induction coil rated at 80 millimeter spark. The primary of this coil was supplied with a current of ten amperes at fifteen volts. The interruptions of the primary current were produced by an attachment to the Schumann chronograph, described below, and the break-spark alone was used, the make-spark being shunted out of the stimulus circuit by the automatic cutout described in the REVIEW of Volume XVII., page 321.

An arm was attached to the drum-shaft of the Schumann chronograph, so that at each rotation it struck the lever of a small contact switch mounted on the frame, thus opening the primary circuit of the stimulus coil, as mentioned above. The drum was rotated by a Martin motor-rotator, the speed being reduced by belting from a small pulley on the motor shaft to a large pulley on the shaft of a fly wheel, and again from a small pulley on this fly wheel shaft to the pulley on the chronograph shaft. A heavy disc of brass was mounted on the motor shaft, adding materially to the steadiness of rotation of the system.

The motor was run on the 110-volt direct current, with lamp resistance in series and sliding rheostat resistance in parallel with the motor. This arrangement, when using a low-wound motor, not only avoids burning the brushes, but also gives more uniform speed, and more efficient speed control than can be obtained by placing the variable resistance in series with the motor.

Motor driving for rhythm apparatus cannot be said to be a great success, and would not have been adopted for this work if it had been possible to use any other system. The records, as detailed below, really show surprising constancy, but many days were wasted on which no records could be made, as the drum could not be made to run at a sufficiently uniform speed on those occasions. I expect to do further work with a pendulum apparatus.

The 250 d.v. fork was used on the chronograph, and the reaction was recorded in the ordinary fashion, by the passage of the spark from the fork-point to the drum. As the make-

spark from the recording induction coil was not of sufficient strength to penetrate the paper, no cut-out was needed in this circuit.

The gears by which the carriage screw of the chronograph is ordinarily driven were removed, and a pulley placed on the screw shaft, with belt to a motor of controllable speed. The movement of the carriage being thus entirely independent of the rotation of the drum, the carriage could be started and stopped without influencing the regularity of the rotation.

The position of the stimulus in the tuning fork record was determined as follows: The drum was so set that the switch controlling the primary circuit of the stimulus coil was on the point of opening. Then the carriage was set in motion, and the point of the tuning fork traced a line across the drum. The distance (in fork vibrations) from this stimulus line to a reaction spark measures the error in the corresponding reaction. To determine if the stimulus spark actually occurred at the moment of the opening of the switch, a number of experiments were made with the chronograph terminals connected in the stimulus circuit, so that the same spark which passed across spark-gap or helium tube also passed from fork-point to drum through the paper. The reaction record coil was of course disconnected from the chronograph for these experiments. The spark record in every case fell directly on the stimulus line. This was true even when the drum was revolved so fast that one fifth sigma was clearly readable. Only when the current was allowed to run in one direction in the primary circuit for over ten minutes was there an appreciable lag to the spark. As in the work described below the current never ran over two minutes without reversal, the practical simultaneity of stimulus with current-break is guaranteed.

Of the apparatus described, the reaction-key, condenser, spark-gap, flashing tube with screen, and *DTDP* switch which controlled spark-gap and tube, were in the reactor's room. The remainder of the apparatus was in the experimenter's room, separated from the reactor's room by another room and a hallway. Not the slightest noise from the operation of the apparatus in the experimenter's room penetrated to the subject's room. The subject's room was darkened during the experiments, except for slight leakage of light under the door.

I desired to employ stimulus rates ranging from  $\frac{1}{4}$  sec. to  $2\frac{1}{4}$  secs. The  $\frac{1}{4}$  rate proved too fast, and even a  $\frac{1}{3}$  rate proved too fast for visual stimulation. These rates were established by counting rotations by the watch, and hence were only approximated. The actual rates were measured later by counting the fork vibrations.

The procedure in the experiment was as follows. The reactor was placed in his room and given a few minutes for adaptation. The chronograph was then set in rotation at approximately the desired rate, and allowed to run for a minute or more, the primary circuits of stimulus and record coils being broken. Then, in order, the tuning fork was set in vibration, the stimulus coil primary circuit was closed, and the record coil circuit was closed. As soon as the reactor began to receive stimulations he prepared to react, beginning his reactions as soon as he caught the rhythm. The experimenter detected the beginning of the reactions by the appearance of the spark at the point of the tuning fork. After about a dozen reactions the experimenter started the fork carriage, so that the recording commenced. After the drum was about half covered, the carriage was usually stopped, and both stimulus and record primary circuits broken, the drum remaining in rotation. After several minutes of rest, the operation was repeated, the currents now being reversed through the coils, and the remainder of the drum covered.

In some experiments on myself I covered the whole of the drum in one operation. I set the drum in rotation, and closed both primary circuits, then took my place in the reaction room and started the carriage from there by closing a switch introduced for that purpose. I had no means of knowing when the drum was filled, but kept on reacting until I was certain that I had exceeded its limits.

The experiments of which the results are given below were done in the spring of 1910, and the measurements of the records was done during the summer. Five reactors were employed; T. A. Lewis, H. M. Johnson and G. R. Wells, graduate students; J. M. Marston, an undergraduate; and myself. The collected results of the experiments are given in Tables I.-V.



TABLE I.  
REACTOR M.

Day.	Order.	Mode.	Type.	Rate.	P. V.	No.	Av. Error.	M. V.
May 18	1a	Vis.	N	1,160	0.3	26	- 66.0	37
	b			1,182	0.3	33	- 56.4	46
	2a	Aud.		1,190	3.2	29	- 63.2	19
	b			1,188	1.3	31	- 50.4	16
May 19	3	Aud.	P	1,032	0.1	31	- 80.4	31
	4a	Vis.		1,006	0.2	20	- 39.6	57
	b			988	0.2	27	- 43.2	29
	5a	Vis.				25	- 108.8	52
May 20	b		P			24	+ 51.4	63
	c					23	+ 24.3	33
	6					23	- 3.8	33
	7a					20	- 30.6	54
	b					32	- 112.7	45
	c					28	- 38.8	68
	8a					22	+ 17.5	48
	b					20	- 126.2	44
	c					19	- 109.8	74
	9a					27	- 6.0	33
	b					31	- 32.0	28
	c					23	- 27.0	37
May 23	10a		N			27	- 2.9	32
	b					27	- 17.0	38
	c					21	+ 23.0	25
	11a					23	- 4.2	28
	b					26	- 19.3	28
	c					18	- 2.5	38
	12	Vis.		963	0.1	61	- 15.8	26
	13	Aud.		973	0.0	65	- 12.4	11
	14a	Vis.		462	0.0	19	- 50.0	19
	b			462	0.0	41	+ 70.8	29
	15a	Aud.		996	0.5	34	- 41.1	27
	b			1,020	0.8	31	- 42.2	28
May 27	16a	Vis.	N	1,034	0.1	33	+ 4.9	42
	b			1,038	0.1	31	- 23.1	25
	17a	Aud.		1,814	0.6	30	- 4.8	46
	b			1,792	0.2	29	+ 4.9	39
	18a	Vis.		1,822	0.7	26	+ 8.3	66
	b			1,810	0.4	29	- 9.2	30
	19a	Aud.		2,354	1.1	28	+ 36.2	64
	b			2,318	2.9	24	- 10.2	68
	20a	Vis.		2,456	3.3	27	- 29.2	51
	b			2,424	0.5	29	+ 34.4	56
	21a	Aud.		2,446	1.1	26	- 42.7	65
	b			2,352	1.6	11	+ 3.6	62
May 28	22a	Vis.	N	2,492	0.4	20	- 110.6	118
	b			2,508	6.0	13	- 42.6	71
	23a	Aud.		1,884	8.8	22	- 98.8	52
	b			2,009	11.5	20	- 51.0	48
	24a	Aud.		1,353	12.7	24	- 74.2	36
	b			1,242	7.4	17	- 5.2	28
	25a	Vis.		1,017	1.1	33	- 50.0	29
	b			1,007	0.5	25	- 4.5	24

TABLE II.  
REACTOR J.

Day.	Order.	Mode.	Type.	Rate.	P. V.	No.	Av. Error.	M. V.
May 19	1a	Vis.	N	1,193	0.3	31	- 16.1	35
	b			1,176	0.2	31	+ 15.8	55
	2a	Aud.		1,103	2.2	23	- 11.3	29
May 20	b			1,138	1.1	32	- 3.8	35
	3a	Aud.		981	0.5	41	- 88.0	21
	b			983	0.9	38	- 55.7	26
	4a	Vis.		963	0.8	22	- 91.4	35
May 23	b			974	0.0	18	- 33.2	30
	5a	Vis.		706	0.2	49	- 13.5	44
	b			711	0.2	42	- 23.8	37
May 24	6a	Aud.		736	0.5	42	- 28.0	7
	b			737	0.0	36	- 29.2	27
	7a	Aud.		524	0.0	30	+ 6.4	11
	b			526	0.1	30	+ 20.2	11
May 25	8a	Vis.		540	0.0	44	- 9.7	41
	b			538	0.1	30	- 39.8	18
	9a	Aud.		326	0.0	40	- 14.4	16
	b			325	0.0	27	- 22.0	9
May 25	10a	Aud.		335	0.0	47	- 12.1	14
	b			348	0.3	39	- 8.4	10
May 26	11a	Aud.		494	0.2	36	+ 18.0	8
	b			502	0.1	30	- 17.0	29
	12a	Vis.		493	0.1	43	+ 7.8	44
May 27	b			490	0.1	30	+ 6.3	31
	13a	Vis.		696	0.0	36	+ 51.4	21
	b			710	0.8	32	+ 23.0	60
	14a	Aud.		737	0.4	29	- 19.8	21
June 9	b			709	0.9	46	+ 24.2	17
	15a	Aud.		548	0.0	27	+ 22.8	9
	b			556	0.0	42	+ 8.3	16
	16a			763	0.0	39	+ 20.3	23
June 10	b			765	0.3	26	- 14.0	15
	17a			1,433	0.2	31	+ 13.0	34
	b			1,416	1.1	29	+ 11.0	43
	18a			902	1.1	40	+ 19.1	17
June 10	b			916	0.4	36	+ 8.4	18
	19a			685	1.1	54	+ 6.0	16
	b			669	1.0	41	+ 1.6	21
	20a			408	0.1	43	+ 2.4	14
June 11	b			414	0.0	38	+ 15.2	11
	21a			454	0.1	36	+ 18.6	10
	b			455	0.0	27	+ 2.4	12
	22a			715	0.4	34	- 12.0	14
June 11	b			705	0.0	29	- 4.6	12
	23a			1,433	0.2	31	+ 13.0	34
	b			1,416	1.1	29	+ 11.0	43
	24a			959	0.2	27	- 31.3	22
June 11	b		S	962	0.0	32	- 32.4	21
	25a		R	959	0.3	28	- 44.7	13
	b		S	968	0.6	26	+ 3.0	31
	26a		S	672	0.1	29	- 7.0	21
June 11	b		R	663	0.0	29	- 5.4	13
	27a		R	656	0.0	35	- 18.9	14
	b		S	659	0.0	32	+ 9.4	13

TABLE III.  
REACTOR L.

Day.	Order.	Mode.	Type.	Rate.	P. V.	No.	Av. Error.	M. V.
June 8	1a	Aud.	N	1,956	0.3	30	-46.6	47
	b			1,956	2.5	26	-45.6	63
	2a			1,253	0.0	34	- 4.1	35
	b			1,269	0.1	31	+19.6	54
	3a			720	0.2	42	+18.8	20
	b			720	0.0	33	+ 4.5	15
	4a			542	0.0	42	+31.4	19
	b			545	0.1	34	+24.6	19
	5a			342	0.0	41	+50.2	24
	b			345	0.2	36	-21.8	12
	6a			352	0.1	38	+16.2	33
	b			354	0.0	—	—	—
June 9	7a			507	0.2	37	+ 8.6	20
	b			510	0.0	35	- 8.2	13
	8a			666	0.0	47	- 8.0	31
	b			651	0.0	39	- 6.4	16
	9a			956	0.8	36	-30.8	21
	b			983	0.4	30	-15.4	25
	10a			1,524	0.2	39	-33.4	34
	b			1,576	1.1	36	-36.4	36
	11a			1,474	0.8	34	-43.4	29
	b			1,494	1.2	31	+ 5.2	35
	12a			978	0.3	44	-30.7	11
	b			960	0.1	32	-22.4	23
June 10	13a			699	0.0	40	-19.7	16
	b			699	0.0	33	+16.8	40
	14a			495	0.1	45	-62.4	16
	b			504	0.0	45	-20.0	11
	15a			327	0.0	45	-15.2	12
	b			329	0.0	36	- 3.7	9
	16a			337	0.0	39	-14.8	19
	b			336	0.0	19	-12.9	10
	17a			477	0.0	28	-14.5	11
	b			477	0.0	24	- 8.2	15
	18a			628	0.0	29	-20.4	10
	b			626	0.0	27	-10.8	11
June 11	19			974	0.6	38	-47.2	21
	20a			1,472	0.1	41	-66.9	30
	b			1,516	0.1	27	+ 4.1	16
	21a			1,022	0.6	26	- 4.1	31
	b			1,025	0.5	29	+ 6.8	37
	22a			1,047	0.7	27	-16.3	24
	b			1,037	0.6	22	-33.8	19
	23a			1,072	0.5	29	- 6.4	26
	b			1,081	0.0	27	+12.3	37
	24a			1,122	0.7	26	+ 7.2	25
	b			1,128	0.3	24	- 0.6	36

No explanation is needed for columns 1, 2 and 3 of the tables. The letters in column 4 indicate the conditions of the experiment. *N* indicates that the subject was given no specific instructions as to attention, except that he was told to make the

TABLE IV.  
REACTOR W.

Day.	Order.	Mode.	Type.	Rate.	P. V.	No.	Av. Error.	M. V.
June 7	1a	Aud.	N	1,261	0.4	43	-68.3	42
	b	Vis.		1,262	0.5	38	-28.0	63
	2a	Vis.		1,298	0.1	40	+ 13.2	47
	b	Aud.		1,346	0.7	40	- 47.2	50
	3a	Aud.		1,272	0.9	38	- 73.5	57
	b	Vis.		1,342	0.7	37	- 16.8	39
	4a	Vis.		1,328	0.6	38	- 41.4	36
	b	Aud.		1,372	1.5	30	- 6.7	32
June 13	5a	Aud.		717	0.2	29	-107.9	31
	b			714	0.0	24	- 81.6	21
	6a	Vis.		727	0.2	42	-172.8	38
	b			719	0.1	28	-172.0	33
	7a	Aud.		722	0.2	36	- 41.2	32
	b			718	0.1	39	- 73.6	32
	8a	Vis.		729	0.0	29	- 94.8	54
	b			725	0.2	31	- 30.4	73

touch of the finger on the key synchronize with the stimulus. Introspection on this point was not asked until late in the experiment, but indicated no conscious inequality of distribution of the attention between the stimulus and the reaction.

*S* indicates that the subject attempted to force the attention on the stimulus as much as possible, and *R* indicates the reverse *i. e.*, emphasis of attention on reaction.

*D* indicates that the attention was distracted from both stimulus and reaction. This was accomplished by having the reactor start with some number of two digits, and add to it successively the odd numbers beginning with one. This addition was commenced when the reactions commenced, and was a highly successful means of distraction.

*P* indicates practice. In these series the reactor was called in at the end of each group (*a*, *b*, *c*), to examine his record, and was then urged to correct the errors in succeeding series. In series marked with letters other than *P* the reactors did not see their records until entirely through with their part in the experiment. I myself was unaware of the nature of my records until after the close of the experiment, although I removed the records from the drum and varnished them, as close scrutiny was necessary in order to detect the reaction positions, and I avoided scrutinizing my records until I was entirely through.

TABLE V.  
REACTOR D.

Day.	Order.	Mode.	Type.	Rate.	P. V.	No.	Av. Error.	M. V.
May 23	1a	Aud.	N	1,004	0.0	45	-40.0	53
	b			1,016	0.0	34	- 9.6	46
	2a			684	0.0	30	-57.9	30
	b			664	0.0	28	-59.0	27
	3a			502	0.0	24	-58.0	16
	b			504	0.0	33	-38.2	14
	4a			342	0.0	36	-74.6	21
	b			340	0.0	44	-71.5	17
	5			1,106	0.0	56	+16.4	29
	May 25			6	Vis.	N	236	0.0
7		Aud.	250	0.0			87	-11.6
8		Vis.	348	0.0	—		—	—
9		Aud.	338	0.0	70		-41.8	18
10a		Vis.	492	0.0	25		+45.6	29
b		493	0.0	32	+ 7.1		18	
11		Aud.	507	0.2	59		-32.2	10
12		Vis.	747	0.0	39		-20.8	25
13		Aud.	709	0.1	46		+ 2.2	17
14		Vis.	947	0.6	32		+48.6	34
June 9	15	Aud.	N	1,114	0.1	61	-35.5	17
	16	Aud.		396	0.0	70	-57.7	14
	17	853		0.1	58	+ 7.8	23	
	18	1,836		0.0	72	-20.8	22	
	19	712		0.1	42	-10.1	10	
	20	397		0.1	53	-34.1	10	
	21	713		0.0	61	-29.9	16	
	22	910		0.2	69	-56.2	20	
	23	696		0.1	51	-38.0	13	
	24	351		0.0	64	-52.7	15	
June 10	25	D	D	363	0.0	56	-27.6	37
	26			662	0.1	45	-53.0	17
	27			1,031	0.2	64	- 7.4	29
	28a			1,129	0.7	22	-32.8	34
	b			1,116	0.4	20	-36.8	46
	29a			1,098	0.3	24	-35.6	44
	b			1,069	0.7	19	-31.6	52
	30a			1,054	1.0	26	-11.4	30
	b			1,035	0.0	26	-28.4	36

The rates are given in *sigma*, the figures giving the number of *sigma* between successive stimulations.

The figures given in column 6 are not for the mean variation, but what is much more important, the average differences between successive rotations. The differences in any record are due to a gradual change of speed from the beginning to the end, so that although the figures given are averages, they do not differ materially from the individual differences in the same series. The actual differences between successive rotations were of course not measured directly, as I made no attempts to

get accuracy of measurement below one *sigma*. Only by comparing the first and last rotations of a given series or half-series with intermediate rotations could the variations be discovered. The formula  $(a - z) \div (n - 1)$  gives the average, where  $a$  is the duration of the first rotation  $z$  of the last rotation, and  $n$  is the number of rotations. By comparing  $a$  and  $z$  with intermediate rotations the fact that the change was progressive in one direction could be established. Series with both increasing and decreasing speed are not found, probably because when the speed was near a maximum or minimum the difference between  $a$  and  $z$  was too small to be noted at all.

The mean variations are approximately 5 to 8 times the magnitude of the indicated p.v. For example, the m.v. of series 1a, Table I., is 1.9 sigma. The m.v. might be computed from the p.v. if it were worth while. For even values of  $n$  the formula is  $m.v. = n(p.v.) \div 4$ .

The figures in column 7 give the number of reactions in a given series, and the figures in columns 8 and 9 give the average errors and mean variations of the reactions in *sigma*. The *plus* sign in column 8 indicates that the reaction occurred after the stimulus, and the *minus* sign indicates that the reaction occurred before the stimulus with which it was supposed to synchronize.

Series 5-11, inclusive of Table I., were given at rates as near 1,000 sigma per rotation as could be established by counting rotations. It was not deemed worth while to go through the tedious process of determining the exact speeds by counting vibrations in these records.

In 6b, Table III., 6, 8, Table V., the rate was faster than the reactor could manage, and he reacted at a rate somewhat slower than the stimulus rate, passing thus progressively from an apparent positive error to an apparent negative error.

A visual record, corresponding to 23, Table I., was made, but accidentally destroyed before it had been measured.

The mean variation of the reactors' errors is seen to be very large throughout the records, even larger than are usually found in the complication experiment.<sup>1</sup> But we would expect to find

<sup>1</sup> Dunlap, PSYCHOL. REV., XVII., 160-165. Burrow, N. T., PSYCHOL. MONOGRAPHS, XI., No. 4, 32.

a larger variation in the finger reaction, because all the reactions are recorded and enter into the computation of the average error and mean variation, whereas in the complication experiment each record is of a judgment based on a number of reactions, which are themselves unrecorded. In this latter case the reactions which give conscious asynchronism may be disregarded by the reactor. In the former case they are unavoidably included in the total.

The consciously asynchronous reactions result largely from the drifting tendency which is exhibited both in the finger reactions and in the complication experiment. The reactor not only may be reacting a little before the stimulus, but he may be reacting at a rate a little faster than that of the stimulus. Each successive error in this case will be a little greater than the preceding, until a point is reached at which the discrepancy is perceived. Thus Geiger says: "Auch während der sonstigen Versuche hatten mir die Beobachter oft ungefragt erklärt, bei jeder Umdrehung scheint der Schall rückwärts zu rücken."<sup>1</sup> Other experimenters have found the same drifting tendency in the complication observation, but it is not always in the negative direction. In the finger reaction records I am reporting here, the drifting is strikingly apparent.

After drifting ahead or back, as the case may be, until perceptible asynchronism is reached, the reactor might make a correction by suddenly returning to a position nearer the stimulus (speaking in terms of the record), interrupting the reaction rhythm and beginning afresh, as it were. This correction actually takes place in but relatively few cases. In the majority of cases the reactor slightly retards the rate (or accelerates it, as the situation demands), and begins to drift in the other direction; but the drift is usually more irregular just after the turn, than just before. Often, after drifting to a maximum, the succeeding reactions fall scatteringly just inside the measure of the maximum.

The following series of reactions, chosen at random, are quite characteristic. The errors of successive reactions are given in sigma.

<sup>1</sup>Geiger, M., 'Neue Complicationsversuche,' *Philos. Studien*, XVIII., 396.

(Portion of 2*b*, Table IV.) -8, -30, -48, -134, -112, -126, -178, -128, -178, 128, -86, -112, -64, +28, +34, +34, -14, -48.

(8*a*, Table IV.) +10, -26, -76, -74, -76, -60, -10, -24, -90, -112, -158, -128, -176, -192, +48, -258, -212, -160, -98, -42, -98, -42, -144, -156, -72, -42, -84, -72, -124.

(First portion of 2*i*, Table V.) -18, -36, -60, -72, -84, -52, -38, -20, +16, -16, -21, -22, -8, +3, 0, -28, -58, -66, -65, -64, -57, -20, -33, -34, -28, -44, -53, -16, +8, -6, -20, -33, -46, -80, -101, -84, -56, -24, -11, -3.

Although there were in each series reactions which the reactor knew to be asynchronous with the stimulus, there was no way of excluding these reactions from the reckoning. It is impossible for the reactor to keep track of his reactions and even if it were possible, the reactions would be rendered worthless by such procedure. The experimenter has no right to cut out any reactions, even although it may be clear from the record that they are abnormal. It is quite evident that in series 8*a*, Table IV., given above, the reactions from -176 to -160 should be rejected. The reactor probably felt -176 as asynchronous, and made an abrupt break in the rhythm after the next reaction (under such circumstances it is difficult to make a change until one reaction after the one felt as asynchronous), beginning with an error of +48 and then jumping ahead to -258, at which point the rhythm was reestablished, but somewhat retarded for the next four reactions. If the six reactions indicated could be thrown out, the average error would be reduced about 17 per cent., but the mean variation would be reduced 50 per cent.

The mean variation is, as we might reasonably expect, independent of the average error; on the whole, the mean variation is as large where the average error is small as where there is a large average error. The mean variation is also as great, on the whole, at the faster rates as at the slower; this is rather curious, as it makes the shorter intervals relatively more irregular than the longer.



In the early experiments on the complication problem—notably those made by Wundt—it was apparently found that the error, usually negative for the slower rates, became less negative, or even positive, for the more rapid rates. Later investigations raised the suspicion that this progressive change in the error was in large part due to mere practice, the natural method being to commence with the slower rates and proceed to the faster. Gieger then attempted to separate the two factors of rate and practice, and showed clearly that for *one subject*, when the effects of practice were practically eliminated, the errors (all negative) were smaller in absolute value for the faster rates. In the cases of several other subjects he showed that practice could change the error from an original positive to a negative value. As yet we have no unexceptionable evidence that the *direction* of error is affected by rate, and we have no reason to doubt that a subject whose errors for slow rates are positive in the beginning might show a decrease in the absolute value of the error with increasing rate.

In the results of the finger reactions, as given in tables above, we find what is usually found in the complication experiment: that the magnitude and direction of the average error, although extremely variable, have no very definite relation to rate of stimulus. That under better conditions of experiment such a relation might appear is quite possible.

No very definite practice effects are discoverable in the tables, except in the case of Reactor J., who, in spite of the positive half of Series 1, is strongly negative for the first three days, becoming thereafter more positive. Reactor L. was the 'Subject III.' in Burrow's experiment<sup>1</sup> who made positive errors almost exclusively. Although there is a large negative average for the first record of this reactor, he shows a positive tendency in the first day's work, but becomes predominantly negative thereafter.

The oriented practice in the case of Subject M. produced rather interesting results. Immediately after the last practice series the averages are remarkably good. (Series 12 and 13, Table I.) The reactions in the first half of the following series

<sup>1</sup> Burrow, *op. cit.*, 31.

were felt to be anticipatory, hence the over-correction in the second half. This in itself shows an increased accuracy of observation, if not of motor performance. The increase in accuracy persisted through the third day after the end of practice, although the reactor had not attained a high degree of skill. On the following day (May 28) there was much confusion. It is evident that the limits of unnoticed asynchronism are capable of being considerably narrowed by appropriate practice.

The few series taken with emphasis of attention alternately on the stimulus and on the reaction produced results exactly similar to those produced under corresponding procedure in the complication experiment. In series 21-24 of Table III. attention to the stimulus gave a more negative error than did attention to the reaction, the signs even being opposite in three series. In series 24-27 of Table II. just the opposite effect was produced in three out of the four cases, and the same is true of series 28-30 of Table V., where the differences are less pronounced, even negligible. That changes in the direction of attention do in some cases influence the reactions is clear, and so far we have no more clue as to the mechanism by which the influence is exercised than we have to the corresponding mechanism in the complication experiment. It is quite probable that with many subjects no effect would be produced by emphasis of the attention to the one factor or the other, since the effect is frequently absent in the complication experiment. In fact, when Reactors L. and D. were tested with differential direction of attention in my experiment on the complication phenomenon, neither showed the slightest influence of this factor.

The use of two modes of stimulation was intended to furnish data, if possible, for the determination of the source of the usual delay of the simple visual reaction, as compared with the simple auditory reaction. If the delay should be due to the greater persistence of the visual sensation, as compared with the auditory, the rhythmic visual reaction might or might not show similar delay, but if the cause should be what we may loosely call motor inhibition, we may reasonably suppose that

the visual rhythmic reaction would show no more positive (or less negative) error than the auditory, since the reaction is not to the stimulus with which it synchronizes, but to the series of stimuli which precede it at relatively large intervals. In any event, the important point is to ascertain if the reactions to the two modes of stimuli do or do not show a characteristic difference.

In spite of the ill-success in the obtaining of auditory and visual series at exactly the same rates, the comparison of corresponding series in the tables above is very interesting. Bearing in mind that a relative delay in the visual reaction would give an algebraically greater (that is, more positive or less negative) error, we can conveniently compare series by noting the excess of the average error for the visual series over the average error for the auditory series.

In Table I. the comparison gives: 1 and 2,  $-4.4$ ; 3 and 4,  $+39$ ; 12 and 13,  $-3.4$ ; 15 and 16,  $+32.5$ ; 17 and 18,  $-0.5$ ; 19 and 20,  $-10.4$ ; 21 and 22,  $-57$ . In general therefore these series indicate no definite difference for the two modes of stimulation.

In Table II. we find the following differences: 1 and 2,  $+7.4$ ; 3 and 4,  $+9.5$ ; 5 and 6,  $+9.9$ ; 7 and 8,  $-38$  (7 being an exceptional series); 11 and 12,  $+6.5$ ; 13 and 14,  $+35$ .

In Table IV. the differences are: 1,  $+40.3$ ; 2,  $+60.4$ ; 3,  $+56.8$ ; 4,  $-34.7$ ; average of 5 and 7 and average of 6 and 8,  $-41.5$ .

In Table V. we find: 10 and 11,  $+58.5$ ; 12 and 13,  $-23$ ; 14 and 15,  $+84.1$ . The  $+2.2$  average error of series 13 should however not be allowed much weight, as every other auditory series at rate near 700 gives a negative error of from 10 to 59.

On the whole, the three reactors of tables II., IV., and V. seem to delay the visual reaction, as compared with the auditory reaction. While the delay is on the average not great it is sufficiently marked to warrant a more intensive investigation of the conditions of the modal factors in the rhythmic reaction.

The general similarity of the subjectively synchronizing

finger reaction to a rhythmic stimulus and the characteristic process in the so-called complication-experiment is pretty definitely shown by the results of these experiments. The reactions, nearly 7,000 in number, were so scattered under various conditions that no great weight of evidence has been accumulated on any of the other points raised. This wide range of the experiment in its preliminary stage was necessary in order that the next stages may be so limited as to be most effective. The problems concerning rate and mode of stimulus, practice with and without orientation, and phase of reaction-movement must be taken up in relative independence, and with perfected apparatus.





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